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**Measuring Neighborhood Effects:
Re-examining the Conceptualization and Operationalization of
Neighborhood Effects**

by

Junia Howell

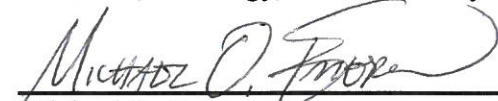
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ABSTRACT

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Junia Howell

Urban sociologists have long studied neighborhood inequality and its implications for residents' life chances. Focusing on marginalized communities, qualitative scholars have illuminated how low educational expectations, destructive social norms and a lack of formal resources limit residents' socioeconomic outcomes. Quantitative scholars then employ these observations to explain the correlations they find between neighborhoods and residents' wellbeing. Yet, the most common measurements of neighborhood effects do not operationalize the multifaceted and nonlinear relationship between residential communities and residents' socioeconomic outcomes. This dissertation is an in-depth investigation into how neighborhood effects are measured and the theoretical and policy implications of these measurements. Organizationally, this dissertation is divided into three empirical studies. The first combines longitudinal geocoded surveys from both the United States and Germany—the U.S. Panel Study of Income Dynamics and the German Socio-Economic Panel—with national censuses, governmental reports and information on local businesses, and finds neighborhood socioeconomic status and institutional resources are not always correlated and operate differently across national contexts. Building off these findings, the second study examines the nonlinear relationship between

neighborhood socioeconomic status and residents' outcomes. Findings suggest neighborhood effects are strongest in advantaged communities. Finally, the third empirical piece in this dissertation examines the tipping points used to classify concentrated poverty. Results indicate the void of poverty—not its excess—drives the relationship between residential context and socioeconomic status. The dissertation concludes with a discussion about the theoretical and policy implications of these findings.

Acknowledgments

I don't have many memories from kindergarten but one is forever etched in my mind. It was parents' night at my school and I proudly walked my mother and father around my classroom pointing out my desk, the math beads, the puzzles, the pitcher and then the books. I explained, "These are the books we read." My mother questioned, "You are reading *these* books?" I can vividly remember standing in front of those teal book bins, embarrassed—not because of anything my mother said or did, but because I knew all my classmates were reading these riveting stories about how Sam sat on Pat and Pat sat on a mat while I still struggled to master my letters. No one had told me I was supposed to be reading. No one made me feel bad about being behind. Yet, even at five years old, I wondered whether I was stupid because I could not read or write my alphabet. I am not sure what my five year old self would think of this completed dissertation but I do know she would not believe it possible. In fact, it wasn't possible without a long series of people believing in me when I didn't—couldn't—believe in myself.

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Preface

Having completed another draft of my third and final dissertation chapter, I lay down, exhausted. As I drifted off to sleep, the sound of rapid gunfire outside my window jolted me awake. I knew it was closer than usual, within 100 yards. I held my breath waiting for the retaliating fire to come. Within moments, they came: six shots, one after the other. Then silence fell. I lay there, too tired to peek out my window or text my neighbors to see whether they knew what was going down. I just lay there, frozen, waiting to see whether sirens would follow. Reassured by the continued silence, I concluded that no bystanders were seriously injured; the shots were not from the police; nothing out of the ordinary had happened. It was just another shootout—an increasingly common occurrence in my neighborhood. As I settled back into my covers, I began to reflect on my dissertation.

I grew up in a neighborhood much like my current community—disproportionately poor, majority Black, with subpar public schools and visible illegal activity. I have lain in bed countless nights calculating the approximate distance of

gunfire and waiting to see whether it warranted alarm. I have watched my childhood playmates—who once denounced their mothers’ residential choices and dependence on family and the government—grow up and find themselves and their own children in much the same situations as their mothers. I have witnessed numerous charismatic community developers and politicians come swooping into communities with grand ideas, only to leave a few years later with little real change accomplished. I have known and continue to rediscover that the confounding structural conditions within urban impoverished communities are detrimental, intergenerational and multifaceted. Nevertheless, I started this dissertation to learn more, to better understand the communities I call home and hopefully, in a small way, bring reform and justice to American cities. Yet, as I lay in bed thinking about my nearly complete dissertation, I was once again baffled by my findings and how different they were from what I anticipated when I proposed this project.

Introduction

I was first drawn to urban sociology as an undergraduate student because of its ability to expose the detrimental effects of neighborhood poverty on generational hardship. When I read Elijah Anderson's (1990; 1999) gripping ethnographies, I could have sworn he was discussing my childhood neighborhood. For the first time, I was introduced to academic research that exposed and explained the structural constraints I witnessed as a child. I then became obsessed with William Julius Wilson's books (1987; 1996) as I tried to better comprehend the historical and contemporary policies that led to concentrated poverty and its harmful effects. I, like these scholars, assumed concentrated poverty was problematic and needed to be addressed. Thus, my aim for my dissertation was to compare city-level policies and features that proliferated concentrated poverty. In scoping outward, I hoped to illuminate new ways cities could address the harmful effects of concentrated poverty.

To ensure my city comparisons were robust, I wanted to analyze a wide range of cities with various policies and demographic features. The desire to compare diverse cities was rooted in the literature's finding that disadvantaged neighborhoods and their effects are distinct depending on the city in which they are located. For example, Chicago's disadvantaged neighborhoods often lack both institutional resources and human capital, while New York's disadvantaged neighborhoods have more business and educational resources despite similarly low human capital (Small 2008). Moreover, these city-level differences result in divergent neighborhood effects on socioeconomic mobility (Sharkey 2013). Additionally, these differences are even more noticeable when cross-national comparisons are utilized. Specifically, European studies have demonstrated that neighborhood effects are not as consequential in Europe as they are in the United States (Drever 2004; Musterd 2005; Arbaci 2008; Arbaci and Malheiros 2010). Given their findings, these European scholars surmise federal and local redistributive policies are more influential on residents' outcomes than their residential location. Nevertheless, to date, no one has conducted direct empirical examinations of neighborhood effects in a cross-national context.

Hence, I designed my dissertation to be a cross-national comparative study that explored the role city-level policies and features had on the intergenerational transmission of socioeconomic status. The initial design focused on city-level factors. Residential neighborhoods were merely a mediating factor to help illuminate the relationship between cities and intergenerational socioeconomic mobility. Yet, as I began to conduct my empirical research, I quickly became dissatisfied by the available neighborhood measures. The existing measures were unable to capture the theoretical

complexities within the literature and the empirical variation between my national case studies. One thing led to another and instead of a dissertation on city policies and features, this dissertation evolved into an in-depth investigation of how neighborhood effects are measured and how this measurement influences theoretical conceptions of neighborhoods and their associated policy interventions. This exploration required me to challenge my own assumptions about concentrated poverty and reexamine the premises of urban sociological theory.

The result is a dissertation that consists of three distinct empirical studies. Each explores a different aspect of how neighborhood effects are measured and the implications of this measurement on theory and policy. The first chapter, “Neighborhood Effects in Cross-Nation Perspective: A Longitudinal Analysis of Impacts on Intergenerational Mobility in the United States and Germany,” directly compares neighborhood effects in the United States versus Germany—two countries with divergent social welfare policies. Moreover, this chapter uses innovative measures of neighborhood effects to differentiate how various neighborhood factors are contributing to observed phenomena. Specifically, this chapter argues that while scholars often evoke two distinct neighborhood mechanisms—neighborhood socioeconomic status and neighborhood institutional resources—in their explanations of neighborhood effects, they rarely employ both in their quantitative studies.

That is, scholars argue that neighborhood socioeconomic status shapes the informal networks, norms and expectations of a community. In turn, these networks, norms and expectations influence children’s educational aspirations and information regarding employment opportunities (Mayer and Jencks 1989; Brooks-Gunn et al. 1993;

Turley 2003; Stewart, Stewart and Simons 2007; Nkansah-Amankra 2010; Martens et al. 2014). Additionally, neighborhood institutional resources (e.g. neighborhood schools, transportation infrastructure, grocery stores, restaurants, and retail shops) impact children's abilities to obtain quality education, acquire fresh food, connect with employment and commercial activities, and travel to and from other areas of their town or city (Freeman 2006; Kimbro, Denney and Panchang 2012). Access to these vital resources in childhood is presumed to play forward to influence residents' subsequent economic opportunities in adulthood. However, when operationalizing these mechanisms, scholars often use neighborhood demographic factors as a proxy for both mechanisms. In this study, I take an alternative approach. I create separate factors—one for neighborhood socioeconomic status and one for neighborhood institutional resources.

Results illuminate how, in the United States, both neighborhood socioeconomic status and institutional resources perpetuate economic hardships from one generation to the next. Conversely, in Germany, neighborhood institutional resources do not influence intergenerational income mobility. Yet, like in the United States, neighborhood socioeconomic status in Germany does effect the transmission of socioeconomic status across generations. These results underscore the importance of considering how broader political contexts foster neighborhood effects on intergenerational mobility in ways that might alter proposed policy interventions. Additionally, they illuminate the theoretical and methodological implications of neighborhood effects measurements. When neighborhood demographic features are used as a proxy for all neighborhood conditions, research is unable to differentiate the role various factors play in intergenerational socioeconomic mobility.

As I was attempting to differentiate these various factors, I discovered that, although many aspects of the neighborhood indicators differ—especially across nations, one surprising finding remained consistent: neighborhood effects measures do not have a linear relationship with the transmission of socioeconomic status across generations. Moreover, unlike the argument put forth by Wilson’s (1987) monumental work, *The Truly Disadvantaged: The Inner City, The Underclass, and Public Policy*, it was not disadvantaged neighborhoods but advantaged neighborhoods that have the strongest influence on residents’ outcomes. The consistency of this finding across countries, outcome variables and neighborhood indicators piqued my interest, and the second chapter of this dissertation was born.

Titled “The Truly Advantaged: Re-conceptualizing the Implicit Neighborhood of Neighborhood Effects,” this chapter discusses the dissonance between the theoretical explanations of neighborhood effects and how they are measured. Scholars overwhelmingly focus on disadvantaged neighborhoods when explaining the theoretical relationship between neighborhoods and residents’ physical, emotional and socioeconomic wellbeing. They argue that the social norms, networks and expectations in disadvantaged communities prevent students from excelling in school and adults from pursuing jobs in the formal economy (Ainsworth 2002; Ainsworth 2010; Casciano and Massey 2012; Brattbakk and Wessel 2013). Nevertheless, when scholars utilize quantitative data to establish the existence of neighborhood effects across various communities, they use the entire spectrum of neighborhoods without considering the possibility of a non-linear relationship.

If, in fact, disadvantaged neighborhoods were the central driver of the relationship between neighborhoods and residents' outcomes, the potency of neighborhood effects would be strongest in disadvantaged communities. In my second dissertation chapter, I test this presumption and find that not only is it false, but the direct opposite is true. Neighborhood effects are strongest in advantaged neighborhoods. To ensure this finding was not a product of how I was operationalizing neighborhood effects, I utilized the most common measure of neighborhood disadvantage in the literature—a proxy using the proportion of the neighborhood that is Black, the proportion of the neighborhood living in poverty, and the proportion of the neighborhood households headed by single parents. Additionally, I restricted the paper to just the United States to speak directly to the bulk of the literature. However, my results are consistent with those I found in my cross-national comparison in which I used the more nuanced neighborhood indicators. The article concludes with a discussion of findings and argues that both academic research and policy interventions attempting to address the issues associated with disadvantaged neighborhoods need to consider the compounding privileges in advantaged communities.

Building off these findings, I began to investigate the specific role that concentrated poverty has on residents' socioeconomic outcomes. Concentrated poverty has become shorthand for the negative implications of neighborhood effects on residents. In turn, demographic researchers examine temporal and regional trends in concentrated poverty to illuminate the factors driving this phenomenon. Most often they measure concentrated poverty as a dichotomous variable in which poor neighborhoods are defined as communities with at least 40 percent of the population living at or beneath the federal poverty line. The presumption is that the negative correlation between neighborhoods and

residents' outcomes is more potent and detrimental for these extremely poor neighborhoods. Nevertheless, to date, no empirical evaluations have demonstrated that this 40 percent tipping point differentiates quantifiably and categorically distinct neighborhoods that have distinct influences on residents' wellbeing.

Hence, my third and final chapter, "The Tipping Point: Examining and Evaluating Demographic Measurements of Poor Neighborhoods," compares over 57,000 different operationalizations of neighborhood poverty. Findings suggest that defining poor neighborhoods as all communities with a poverty rate of at least 5 percent captures the most variation in educational attainment. Noting this result is vastly different than the traditional tipping point of 40 percent, I run a series of supplemental analyses. All analyses confirm the finding that educational attainment is driven by the void of neighborhood poverty not the abundance of it. I then take these findings one step further and conduct demographic analyses of trends in metropolitan level poverty concentration.

Demographic reports and studies on concentrated poverty make important contributions to scholarly and policy understandings of which metropolitan areas have particularly high levels of poverty concentration. These studies rely on the traditional definitions of concentrated poverty to calculate the proportion of metropolitan residents that live in "impoverished" neighborhoods. Although it is possible that using an alternative measure of poor neighborhoods would not change the conclusions of these studies, it is also possible that changing how neighborhood poverty is defined would radically shift the conclusions drawn about metropolitan poverty exposure. To examine whether the alternative definition of poor neighborhoods (all communities with at least 5 percent poverty) alters trends in metropolitan proportions living in poor neighborhoods, I

compare traditional and alternative measures. I demonstrate that temporal trends and correlations between metropolitan characteristics and poverty concentration differ depending on the measure of neighborhood poverty. These findings demonstrate how measurements have direct and influential implications on scholars' empirical conclusions, theoretical explanations and policy implications.

Taken together, these three empirical studies question conventional neighborhood effects measurements and in doing so, the theoretical conceptions of neighborhoods themselves. The first chapter, "Neighborhood Effects in Cross-Nation Perspective", illuminates how the effects of neighborhoods are not an inevitable outcome of certain demographic or structural features. Instead, neighborhoods are multifaceted and their effects on residents are shaped by their larger national and political context. The second and third chapters, "The Truly Advantaged" and "The Tipping Point", empirically demonstrate neighborhoods that influence their residents most are those with limited poverty and compounded advantage. These results suggest scholars should illuminate how neighborhood indicators are multifaceted and focus on the influence of privileged neighborhoods. This shift in perspective also requires a theoretical shift that conceptualizes the role of average and privileged spaces as central in scholars' discussions of urban inequality and policy interventions.

I conclude the dissertation by expanding on the theoretical and policy implications of this research. In my conclusion, I propose a critical urban theory of neighborhood effects that borrows from Critical Race Theory to reexamine and reimagine how urban scholars can study urban space and inequality. Just as Critical Race scholars have questioned the implicit normality of Whiteness to illuminate its pervasive

dominance, I question the implicit assumptions of urban sociology and their unintentional reproduction of privilege. I invite scholars to think critically and creatively about how neighborhoods are evaluated.

In the end, this dissertation is not explicitly about the violent crime, lack of resources or persistent poverty in the marginalized communities around the world. It is not the dissertation I once imagined presenting to my neighbors and friends about how cities could minimize the negative effects of concentrated poverty. Instead, this dissertation takes an in-depth look at neighborhood measurement and the presumptions in the literature. In doing so, this thesis indirectly addresses the prevalent issues associated with urban poverty by illuminating the role privileged communities have in the perpetuation of inequality.

Chapter 1

Neighborhood Effects in Cross-Nation Perspective: A Longitudinal Analysis of Impacts on Intergenerational Mobility in the United States and Germany

1.1. Executive Summary

Research in the United States provides evidence that neighborhood conditions affect intergenerational mobility. However, what remains unclear is the extent to which the U.S. context is unique in producing this influence. To examine this question, the present study directly compares different dimensions of neighborhood effects on intergenerational mobility in the United States versus Germany—a country whose social welfare policies differ significantly from those in the United States. Results illuminate how, in the United States, residential segregation along socioeconomic lines conjoins with unequal

allocations of neighborhood institutional resources to perpetuate economic hardships from one generation to the next. Results also indicate that the degree and nature of this process depends on the larger political system within which it is embedded.

Neighborhood effects on intergenerational mobility, in other words, are nationally specific. These findings underscore the importance of considering how broader political contexts foster neighborhood effects on intergenerational mobility in ways that might alter proposed policy interventions.

1.2. Introduction

Research in the United States consistently demonstrates that residents of disadvantaged neighborhoods have access to fewer resources and opportunities than their counterparts living in more advantaged communities. This deficit not only influences the wellbeing of adults who live in disadvantaged neighborhoods but also the life chances of their children, thereby contributing to the intergenerational transmission of economic hardship among less-advantaged residents (DuBois 1996[1899]; Condran and Denton 1987; Wilson 1987; Elliott et al 2006; Wodtke, Harding and Elwert 2011; Massey, Sampson 2012; Sharkey 2013; Chetty, Hendren and Katz 2016). By contrast, studies in Europe find little or no such “neighborhood effect,” either on the economic wellbeing of adults or across one generation to the next (Maloutas and Karadimitriou 2001; Musterd and Deurloo 2002; Brannstrom 2004; Arbaci and Malheiros 2010; Bolt, Phillips and Van Kempen 2010; Phillips 2010; Weeks et al. 2010; Andreotti, Gales and Fuentes 2013). This body of research generally posits that such cross-national differences in

neighborhood effects exist because, in contrast to the United States, federal redistribution systems in Europe provide adequate resources to all residents in *all* neighborhoods. This more universal assistance is presumed not only to help level the spatial playing field but also to limit residents' reliance on uneven neighborhood resources that may still remain, thereby minimizing neighborhood effects on the transmission of economic status from parents to children (Veldboer, Kleinhans and Duyvendak 2002; Musterd 2005).

On its face, this line of explanation certainly seems plausible. Yet, to date, it has been largely presumed rather than rigorously tested through direct, comparative research. A primary goal of the present study is to provide this direct comparison. In the process, I also seek to illuminate how two distinct dimensions of neighborhood effects—those associated with socioeconomic status and those associated with institutional resources—while often conflated, operate differently in each national context. To conduct this research, I employ a cross-national, longitudinal design to answer the following questions: How do neighborhood resources during childhood affect intergenerational income mobility? And, how does the answer to this question vary across societies with similar affluence and demographic characteristics but different social welfare policies? Similar to previous neighborhood-effect studies, this design links neighborhood-level data to geocoded, longitudinal data on individuals and families, using comparable, restricted data from the United States' Panel Study of Income Dynamics (PSID) and Germany's Socio-Economic Panel [Sozio-oekonomische Panel (SOEP)]. Using a cross-national comparative analysis enables me to directly test the extent to which larger social and political contexts shape neighborhood effects on intergenerational economic mobility

and in turn illuminate new policy alternatives for reducing the intergenerational transmission of economic hardship.

1.3. Neighborhood Effects: Socioeconomic Status and Institutional Resources

Scholars concur that U.S. children who grow up in disadvantaged neighborhoods have fewer economic opportunities and experience less mobility than children in more advantaged communities. Yet, they debate whether this correlation is due to parents' "selection"¹ of neighborhoods or the structural conditions of neighborhoods themselves. On the whole, empirical studies have concluded that both dynamics are typically in play (Massey, Condran and Denton 1987; Wilson 1987; Elliott et al 2006; Sampson 2012; Sharkey 2013; Chetty, Hendren and Katz 2016). That is, in the United States children raised in disadvantaged neighborhoods² have restricted economic opportunities because (a) their families tend to have fewer resources to invest in their children to begin with

¹ Using the word "selection" to discuss neighborhood location harkens back to the Chicago school, who posited that marginalized groups choose to self-segregate. Scholars have discredited this assertion and illuminated the structural conditions constraining marginalized populations' residential choices. For congruency with the neighborhood effects literature, I utilize the word "selection" but do *not* concur with its original insinuation that marginalized actors have sole responsibility for their residential locations.

² The inverse is also true. Children raised in advantaged neighborhoods have *increased* economic opportunities. The ethnographic and theoretical discussions of neighborhoods are primarily centered on disadvantaged neighborhoods. Thus, I pull from this literature in my discussion, yet, this does not exclude the impact that advantaged neighborhoods have on their residents.

(selection effect); and (b) due to the spatial concentration of disadvantaged families, these communities also have fewer social and institutional resources outside the family to assist with children's economic mobility (structural effect). These family and neighborhood level characteristics are presumed to overlap and reinforce one another.

To explain neighborhood structural effects, scholars pull two distinct explanations from ethnographic research. First, scholars conceptualize the neighborhood's socioeconomic status as shaping the community's informal networks, norms and expectations. In turn, these networks, norms and expectations influence children's educational aspirations and information regarding employment opportunities (Mayer and Jencks 1989; Brooks-Gunn et al. 1993; Turley 2003; Stewart, Stewart and Simons 2007; Nkansah-Amankra 2010; Martens et al. 2014). In other words, children raised in an area where the majority of residents are college educated and hold middle-class jobs are more likely to adopt high educational aspirations and gain access to early career building opportunities, even if their parents have low socioeconomic status. Conversely, research has demonstrated that even children of highly educated parents can adopt low educational aspirations and be excluded from informal employment networks when they grow up in neighborhoods of low socioeconomic status (Patillo 2007). In these ways, the socioeconomic status of a neighborhood is presumed to shape informal networks, norms and expectations that in turn influence children's economic opportunities into their adulthood.

The other neighborhood-level explanation, or mechanism, that scholars often invoke is neighborhood institutional resources, including: neighborhood schools, transportation infrastructure, grocery stores, restaurants and retail shops. These formal

resources impact children's abilities to obtain quality education, acquire fresh food, connect with employment and commercial activities, and travel to and from other areas of their town or city (Freeman 2006; Kimbro, Denney and Panchang 2012). That is, even in neighborhoods of comparable socioeconomic status, if the neighborhood public schools are well funded, children will have more opportunities to succeed academically.

Likewise, readily accessible retail stores provide access to needed goods but also early employment opportunities. Access to these vital resources in childhood is presumed to play forward to influence residents' subsequent economic opportunities in adulthood.

Although scholars repeatedly invoke both neighborhood socioeconomic status and institutional resources as distinct mechanisms in their theoretical explanations of how neighborhoods influence intergenerational economic mobility, empirically and methodologically they often conflate the two. That is, they tend to assume that low levels of socioeconomic status imply limited institutional resources and vice versa. Small (2008) has challenged this presumption, noting that correlations between neighborhood demographic factors and institutional resources vary by metropolitan area. Some disadvantaged communities, like those in Chicago, lack institutional resources *and* human capital, while others, like those in New York City, have institutional resources but lack human capital, and still others lack institutional resources but have high levels of human capital. Recognizing this heterogeneity creates room for theoretical and analytical specificities that can have important implications for policy interventions. Moreover, distinguishing these two types of presumed neighborhood effects could help to illuminate empirically why cross-national differences in neighborhood effects exist.

As mentioned previously, unlike U.S. scholars, European researchers have found that neighborhoods have little to no impact on residents' outcomes. This is so despite that fact that, as in the United States, Europe's growing immigrant populations are often concentrated in impoverished ethnic enclaves. Furthermore, these communities are often characterized by inferior education, joblessness and failing infrastructure. Hence, many scholars have assumed that these neighborhoods are negatively impacting residents' integration and intergenerational economic mobility (Veldboer, Kleinhan and Duyvendak 2002; Musterd 2005). Yet, research has found no correlation between these neighborhoods and intergenerational economic mobility (Brannstrom 2004; Musterd and Deurloo 2002; Arbaci and Malheiros 2010).

To help explain these non-findings, researchers have posited that the difference between U.S. and European neighborhood effects is due to their distinct federal redistribution systems (Veldboer, Kleinhan and Duyvendak 2002; Musterd 2005). That is, in both national contexts, neighborhoods' socioeconomic status and institutional resources vary. Yet, in Europe these local neighborhood networks and resources have less impact on children's economic outcomes because the federally distributed governmental services and resources allow residents to access needed resources even when they are unavailable in their residential community. Conversely, many services in the United States are decentralized resulting in unequal allocation and thereby amplifying the importance of local neighborhood resources.

Again, however, as reasonable as this line of explanation is, the lack of direct, cross-national analysis means that, to date, it still remains unclear exactly why observed differences exist. These differences could be due to analytical and operational

distinctions. Furthermore, although European scholars are suggesting that the allocation of federal resources diminishes the influences of both neighborhood socioeconomic status and institutional resources, it could be that federal resources alter the relationship between one of the neighborhood components and not the other, for example, institutional resources and not socioeconomic status. Only direct empirical testing can help us adjudicate these different possibilities.

1.4. Research Design and Cross-National Case Selection

Ideally, an empirical test of whether neighborhood effects differ by national context would include multiple countries with different levels of residential segregation, welfare policies and economic systems. However, the geocoded, longitudinal and neighborhood data required for this analysis is currently only available in a small number of countries and is highly restricted to protect respondents' identities and privacy. Thus, the present study selects two contrasting cases, the United States and Germany,³ whose comparison provides empirical and policy insights unexplored in the literature which can serve as building blocks for future studies.

The United States and Germany were selected because of their contrasting housing markets and federal redistribution systems. Housing markets influence where

³ To use the two countries employed in this study, the process of gaining access to the data took eight months and required multiple compromises on the part of both countries. In the end, both institutions agreed to allow me to analyze all data on site in Berlin as long as the U.S., instead of the German, restricted data procedures were followed.

families live and in turn how neighborhood “selection” affects intergenerational economic mobility. U.S. cities have long had high levels of socioeconomic and racial segregation perpetuated by federal policy and the commodification of housing (DuBois 1996[1899]; Iceland and Wilkes 2006; Dwyer 2010; Sampson 2012). Specifically, federal decisions to underwrite home mortgages, to provide homeownership tax incentives and to foster suburbanization through infrastructural development have created and maintained segregation patterns (Jackson 1985). Furthermore, in the United States, housing is conceptualized as a commodity whose value is closely linked to the surrounding neighborhood. Thus, residents often select neighborhoods that will maximize their investment capital which in turn perpetuates socioeconomic segregation (Peterson 1981). In contrast, Germany has not subsidized homeownership to the same extent as in the United States and housing is largely viewed as a right not a commodity (Veldboer, Kleinhans and Duyvendak 2002). Correspondingly, home ownership,⁴ segregation and residential mobility are all lower in Germany compared to the United States (Musterd 2005). These differences in housing markets might play a role in differential neighborhood “selection” effects.

Beyond their distinct housing markets, Germany and the United States also have distinct approaches to distributing governmental services. Broadly speaking, Germany distributes services federally while the United States allocates services primarily through local communities. For example, in Germany the federal and state (*länders*) governments are responsible for educational financing and make 57 percent of the decisions regarding

⁴ According to the national censuses, Germany homeownership rate is 53 percent and U.S.’ rates is 66 percent.

public education curriculum and leave only 21 percent of the decisions up to the local districts (Klumpp et al. 2014). In contrast, only 24 percent of U.S. educational decisions are made by federal and state governments while 53 percent of the decisions are left to the local districts (Lareau and Goyette 2014). This decentralization has contributed to the high inequality within the U.S. education system. Broadly speaking, the U.S. decentralization of government services increases the power and importance of local governments which might in turn exacerbate the effect of U.S. neighborhood resources on children's educational attainment and even their economic status in adulthood.

Furthermore, no matter their residential location, German residents have access to more comprehensive social safety-net programs and benefit from more inequality regulations than do U.S. residents (Grabka and Kuhn 2012; Muller and Steiner 2013; Grabka and Goebel 2014). Germany spends 15.9 percent of its Gross Domestic Product (GDP) on programs that provide housing subsidies as well as support for elderly individuals, unemployed or disabled workers and impoverished mothers and housing subsidies. In contrast, only 8 percent of U.S. GDP is spent on comparable programs (World Bank 2006). Furthermore, U.S. federal aid programs like Temporary Aid for Needy Families (TANF) are distributed and regulated through state and local governments which have the power to adjust aid amounts and requirements, thereby creating inequalities across regions. Moreover, Germany regulates inequality by setting workers' wage minimums as a proportion of their firm's highest earners (Muller and

Steiner 2013; World Bank 2006).⁵ Unlike the United States, which legislates minimum wages with a set amount, Germany's approach automatically adjusts with the market, curtailing the growth of income inequality.

These differences between the German and U.S. housing markets and wealth redistribution policies make the two countries advantageous cases for comparative research but equally important are their demographic similarities. Like the United States, the German population is both large and spread across multiple urban areas. Germany is smaller than the United States (approximately 80 million compared to 300 million people), but it is the largest nation in Western Europe. Furthermore, Germany—like the United States—continues to experience an influx of non-European immigrants and the expansion of ethnic enclaves. Similar to the Hispanic and Black populations in the United States, immigrant populations in Germany have been met with hostility and are disproportionately concentrated in ethnic enclaves (Simon and Lynch 1999; Fetzer 2000; Ozuekren and Ergoz-Karahan 2010; Glikman and Semyonoy 2012; Sager 2012).

Turks, in particular, have seen dramatic increases in their numbers as well as their social isolation in ethnic enclaves (Kerbo 1996; Ozuekren and Ergoz-Karahan 2010; Glikman and Semyonoy 2012; Sager 2012; Lersch 2014). The first wave of Turkish immigration began in 1961 with the allocation of guest worker permits that were intended to allow contract workers to come for short stints. However, changing economic and political conditions in both countries led to Turks permanently settling in Germany and

⁵ As of January 2015, Germany changed their policy and enacted their first federal minimum wage. Yet, all the data utilized in this research was collected prior to this policy adaptation.

bringing along their families. Now Turks are the largest non-German heritage group in the country with over 2,700,000 people, nearly half of whom were born in Germany to Turkish parents.⁶

Given these demographic similarities and policy distinctions, the United States and Germany are ideal case studies for this research. That is, utilizing these two contexts enables the present study to begin to differentiate the roles played by demographic factors and federal housing and redistribution policies in shaping the relationship between childhood neighborhoods and changes in economic wellbeing from parents to children.

1.5. Data and Methods

This study utilizes two longitudinal geocoded datasets linked to neighborhood demographic and resource data. The longitudinal data sets include: the Panel Study of Income Dynamics (PSID) and the German Socio-Economic Panel [Sozio-oekonomische Panel (SOEP)]. The PSID began collecting annual data on 5,000 households in the United States in 1968. Following these families, their children, and children's children, the sample has now grown to include 9,000 households and over 22,000 individuals. The survey includes questions on employment, wages, income, education, expenditures, and wealth and has been geocoded to U.S. Census Tracts. Modeling off the PSID, the SOEP

⁶ According to the 2011 German Census; the most recent census used in this research.

began in 1984 and now includes 11,000 households (over 20,000 individuals). Like the PSID, the SOEP has been geocoded and linked to German neighborhood data.

The U.S. demographic neighborhood data come from the 1980, 1990 and 2000 Census Summary Tape Files 3 as well as the 2005-2010 American Community Survey. All census tracts were normalized to the 2010 Census Tract boundaries (Logan, Xu and Stults 2014). Data on neighborhood institutional resources come from the annual district reports available on the National Center of Education Resources and Inter-University Consortium for Political and Social Research's 1972, 1977, 1982, 1987, 1992, 1997, and 2002 Census of Governments. All neighborhood data for the years between the governmental and population censuses were linearly imputed.

The German neighborhood data come from Microm—a data collection firm. Unlike the U.S. Census Bureau, the German Census does not collect information on neighborhoods. Researchers who examine neighborhood effects in Germany use data provided by private research institutes—most commonly Microm. In addition to the Microm data, data on neighborhood educational and transportation resources come from the Deutschland Federal Statistical Office. All German contextual data is collected annually.

U.S. census tracts and Microm neighborhood boundaries are created based on dividing streets, train tracks, and natural geographic features as well as residents' understandings of neighborhood boundaries (Lersch 2014). However, the average Microm neighborhood includes fewer residents than U.S. census tracts. This is because German cities are more densely populated and walkable than U.S. cities, meaning residents conceptualize their neighborhoods as covering smaller areas. That said, U.S.

neighborhoods have a wide range of population sizes. In fact, the German neighborhood range is encompassed within the U.S. range. Thus, statistically speaking, including the German neighborhoods is no different than conducting analyses across all U.S. neighborhoods. Yet, in comparison to the United States, the smaller standard deviation for German neighborhood sizes means the German neighborhood effects are likely more precise estimates.

1.5.1. Income Mobility

As previously mentioned, neighborhood resources can influence the wellbeing and life chances of *all* residents. Nevertheless, many neighborhood effects scholars are particularly concerned with the influences that childhood neighborhoods have on intergenerational economic mobility (Sampson 2012; Sharkey 2013; Wilson 1987). A widespread belief in the United States is that all children, no matter their background, should have equal opportunities. Thus, childhood neighborhoods that preclude residents from becoming upwardly economically mobile are perceived to be particularly unjust. Hence, like previous research on neighborhoods, the present study focuses on intergenerational economic mobility.

Following the lead of Bowles, Gintis and Groves (2005), intergenerational income mobility is measured as the *change* between the first and second generations' incomes. To obtain the first, or parental generation's income, I calculated the household per capita income for every year the parents were in the data set and 25 years old or older. Using all adult years, I created an average household per capita income for the first generation. Then, to obtain the second, or children's, income in their adult years, I calculated the

household per capita income for every year the children were in the data set and 25 years old or older. For families with multiple adult children, I average all children's household per capita incomes for every year. I then calculated the mean of this average across all the years the children were 25 years old or older, creating an average household per capita across both time and siblings. Finally, to examine predictors of change, or variation, between the two generations, I use a lagged modeling strategy discussed in more detail below.⁷ Before calculating the average income for each generation, I converted all incomes to 2010 U.S. dollars using U.S. and German Consumer Price Indices. Using the average income across all adult years, does not assume income is permanent across the life-course. Unlike previous mobility literature that focuses solely on fathers and sons, this study averages the income of every respondent in each generation to include daughters and mothers.⁸

⁷ Bowles, Gintis and Groves (2005) take the additional steps of natural logging the incomes and subtracting each generation's income from the overall mean. Using D'Agostino, Belanger and D'Agostino Jr.'s (1990) comparison test of skewness and kurtosis, I concluded that the identity variable is less skewed than its natural logged counterpart. However, the square root of the variable is less skewed than the original. Thus, all models were run with the second generation's income square rooted. Results were comparable and are available upon request. Additionally, supplemental models were run excluding observations whose residuals were three standard deviations above or below the mean (n=62) and no substantive differences were found. For ease of reader comprehension, untransformed coefficients are presented.

⁸ Using generations as the unit of analysis has the added benefit of capturing the reality that resources are rarely—if ever—purely individual (Oliver and Shapiro 2006[1995]). In other words, an individual who chooses a less economically prosperous job (e.g. social work or art) but has siblings who are lawyers, engineers and accountants still has more socioeconomic resources than others making their same income. Conversely, individuals who are the only college educated and/or stable employed members of their extended family often give financial resources, employment advice, and emotional support, curtailing their

1.5.2. Neighborhood Measures

Consistent with the majority of neighborhood effects studies, I created factor variables to capture neighborhood-level phenomena. Specifically, this research utilizes two factors: one for *socioeconomic status* and one for *institutional resources*. The factor for neighborhood socioeconomic status includes the neighborhood's median income, educational attainment, and employment rate.⁹ Neighborhood median income in the United States is pre-taxes and in Germany it is post-taxes, yet both are standardized for comparability. Neighborhood education is the proportion of 25 or older residents with a bachelor's degree. Neighborhood employment rate is the proportion of adults currently employed.

Neighborhood socioeconomic status could be measured as absolute or relative. Although both approaches are valuable, this study utilizes the relative approach to illuminate how neighborhoods—relative to others—influence residents. An advantageous side effect of this approach is that it minimizes the impact of measurement inconsistencies across the two nations. Practically speaking, I calculated the neighborhood socioeconomic status factor for every year. I then averaged the annual neighborhood socioeconomic statuses of the first generation across all the years. I utilized

own ability to save money for their immediate family (Oliver and Shapiro 2006[1995]; Hall and Crowder 2011). Thus, these individuals' overall economic statuses are not equivalent to similar middle-class individuals embedded in middle-class families.

⁹ Previous studies also included the proportion of the neighborhood that was Black or White. The proportion of the neighborhood that was White in the United States and native White Germans was included in all models but was statistically insignificant and not theoretically central to this paper's argument so these results are not presented here.

this composite score because the duration in a particular kind of neighborhood is what matters most for children's economic trajectories (Wodtke, Harding and Elwert 2011; Sharkey 2013; Chetty, Hendren and Katz 2016).¹⁰

Since this composite neighborhood score was created from different decades of annual neighborhood socioeconomic status factors, the Cronbach alphas vary over observed years. Specifically, they range from 0.71 to 0.90 in Germany and from 0.47 to 0.81 in the United States. Conceptually, this cross-national difference suggests that the score is more statistically reliable, or consistent, over time in Germany than in the U.S. That is, respective indicators have tended to covary more consistently with one another. Statistically, this would suggest that the estimated effect of neighborhood socioeconomic status is likely to be more conservative in the U.S. than in Germany. This is because the relative imprecision of the U.S. factor scores are likely to increase estimates' standard errors, thereby reducing chances of statistical significance.

In comparable fashion, I created a factor for neighborhood *institutional resources*. As mentioned previously, institutional resources are commonly evoked to explain neighborhood effects but rarely operationalized. Thus, the present study operationalizes a novel factor based on scholars' discussions of institutional resources. Specifically, I utilize funds spent on compulsory education, commercial commerce, and access to transportation (Freeman 2006; Kimbro, Denney and Panchang 2012). Compulsory education spending was operationalized as total per pupil operational cost in the United

¹⁰ To clarify, I am not examining whether families migrate to "better" or "worse" neighborhoods over time, or if neighborhoods are undergoing gentrification or disinvestment. Instead, I focus on the duration of time spent in particular neighborhood types.

States and teacher to student ratio in Germany. Local level financial reports were not available in Germany. Yet, in both Germany and the United States, teacher salaries are the largest component of operational cost. Thus, Germany's teacher-student ratios are utilized as a proxy for per pupil cost. In both countries, commercial commerce was measured as the number of business establishments per person and transportation infrastructure as the proportion of residents who live without a car.¹¹

Similar to the socioeconomic status factor, families' neighborhood institutional resource scores are computed as the average of their annual scores.. Across observed years, Cronbach alphas range between 0.61 and 0.69 in Germany and between 0.26 and 0.45 in the United States. As with the neighborhood socioeconomic status factor, the lower Cronbach alphas in the United States suggest that assessment of statistical significance will be more conservative in the U.S. than in Germany because of their likelihood of increased standard errors. It also suggests that, in both countries, respective types of institutional resources do not covary as strongly as socioeconomic resources at the neighborhood level. That is, neighborhood access to educational resources may not be a particularly good predictor of neighborhood access to commercial resources or to transportation resources. For this reason, tests of the effects of neighborhood institutional resources are conducted for specific indicators as well as for the composite factor score.

¹¹ The proportion of residents who live without a car is a much better proxy for public transit infrastructure in Germany than in the United States. Other measures such as the mean commute time combined with the proportion of residents that commute on public transportation were better proxies in the United States. Three alternative measures were utilized in the United States, all with higher Cronbach alphas but each produced comparable results (available upon request). Given the consistency in results, this measure was chosen to maximize comparability across the nations.

1.5.3. Familial Controls

Aligning with previous U.S. neighborhood effects research, I control for socioeconomic and demographic characteristics to differentiate familial and individual effects from neighborhood effects. I control for the following: the *years completed in school*, *proportion of years respondents were unmarried*; *proportion of years respondents had children living in the household*; *proportion of years respondents lived in female-headed households*; *number of workers in the household averaged across all adult years*; *age of the respondent when last surveyed*; as well as *age squared*, *number of respondents in the generations*, and the *number of years surveyed*.¹² Descriptive statistics for all variables are reported in Table 1.1.

The models also include parental race. In both Germany and the United States, intergenerational economic hardship occurs more commonly among non-White populations. Yet, it must be noted that conceptions of racial categories are different in Germany than the United States. In the United States, who has been considered White has changed over time, but Whites have always been the political and numeric majority and viewed as superior to all other racial groups (Lopez 1996; Glenn 2004; Feagin 2010; Painter 2010). Blacks, on the other hand, have experienced the most structural and interpersonal discrimination limiting their intergenerational economic mobility. Thus, scholarship has often referred to Blacks as the “exception” to patterns of mobility and integration. The classification of Hispanics, Asians, Native Americans, and multiracial

¹² For each family, each of these controls was calculated for each respondent and then averaged across all the respondents within the generation.

individuals has changed over time and in different parts of the United States, but most recently they are perceived as “other” and are placed somewhere between Whites and Blacks on the racial hierarchy (Bonilla-Silva 2004).

	United States	Germany
Dependent Variable		
Second Generation's Per Capita Income	19,748 (15,821)	17,381 (10,203)†
Neighborhood Effects		
Socioeconomic Status	-0.08 (0.66)	-0.02 (0.54)†
Institutional Resources	-0.06 (0.45)	0.02 (0.88)†
Familial Controls		
<i>First Generation's</i>		
Per Capita Income	13,388 (14,382)	14,872 (10,732)†
Years in School	10.82 (2.58)	11.51 (2.17)†
Majority Race	52.89	87.90†
Exception Race	32.08	3.07†
Other Race	15.03	9.03†
Proportion of Years Single	0.26 (0.34)	0.20 (0.34)†
Proportion of Years with Kids	0.48 (0.31)	0.27 (0.30)†
Proportion of Years Only Female Adults	0.27 (0.36)	0.12 (0.28)†
Workers in Household	1.02 (0.48)	1.38 (0.64)†
Age in Last Year Surveyed	67.61 (12.25)	62.76 (9.09)†
Persons in Generation	2.15 (1.00)	2.05 (0.69)†
Years in Survey	21.65 (9.75)	17.77 (8.43)†
<i>Second Generation's</i>		
Years in School	12.80 (1.93)	12.44 (2.07)†
Proportion of Years Single	0.20 (0.23)	0.46 (0.39)†
Proportion of Years with Kids	0.55 (0.31)	0.27 (0.34)†
Proportion of Years Only Female Adults	0.22 (0.28)	0.16 (0.29)†
Workers in Household	1.28 (0.46)	1.54 (0.65)†
Age in Last Year Surveyed	43.30 (10.77)	33.53 (7.01)†
Persons in Generations	6.50 (4.72)	3.03 (1.97)†
Years in Survey	11.52 (7.41)	7.62 (5.78)†
N	2,868	2,214

† Germany's mean is statistically distinguishable from the United States with a P value ≤ 0.05 .

Table 1.1 – Descriptive Statistics of Respondents in Sample.

Historically, Germany's racial groups were Jews and Gentiles. Nevertheless, the negative political and social replications of World War II have decreased anti-Semitism. Simultaneously, the influx of Turkish immigrants has increased hostility between native Germans and Turks (Steinberg 2001; Lersch 2014). Like Blacks in the United States, Turks are seen as the "exception," often live in ethnic enclaves, and experience limited upward mobility (Kerbo 1996; Ozuekren and Ergoz-Karahan 2010; Glikman and Semyonoy 2012; Sager 2012; Lersch 2014). The perception of native White Germans, on the other hand, is that they are positioned atop the racial hierarchy. Other immigrant and racial groups rank somewhere in the middle. For comparability between the two countries, racial classification is conceptualized as "majority"—Whites in the United States and native White Germans in Germany, "exception"—Blacks in the United States and Turks in Germany, and "others"—all other racial/national identifications.¹³

For most families in the sample, all individuals in the family have the same racial identity, and thus their family's race matches this identity. Yet, for families where this is not the case, parental race was coded as majority race if at least 90 percent of the parents identify as the majority race. If at least 80 percent of the parents identify as the exception race I coded families as the exception race, and I coded the rest as the "Other" race

¹³ To examine whether this conception of race fits the distributions in the data, I operationalized race in a multitude of ways and utilized Vuong and Clarke's non-nested model test to estimate the best categorization. For Germany the alternative classifications included: all Middle Easterns as "exception" category as well as examining a four group classification schema with separated Whites, Turks, Africans and Others. In the United States alternatives included: Hispanics included in the "exception" category with Blacks and differentiating White, Black, Latino, and Other as four distinct groups. For all outcome variables the original three-tiered conception was the best fit.

category. I coded U.S. residents identifying as two races as 0.5 one race and 0.5 the other race, those who named three races as 0.33 of each race. I then utilized these proportions when calculating the family's race. To illustrate how this works, consider a family with two biological parents and one stepparent, two of which identify as Black and one that identified as White and Black. Then the family would be considered 83.3 percent Black and 16.7 percent White. Thus, as a whole the family would be identified as Black—the exception race.

1.5.4. Statistical Modeling

To examine predictors of intergenerational income mobility, I employ a lagged modeling strategy. Change between a first and second generation's income can be modeled using change scores or by predicting the second generation's income while holding the first generation's income constant. These methods are algebraically equivalent and thus produce indistinguishable results (Bowles, Gintis and Groves 2005). For ease of model interpretation, I employ the lagged models. That is, I predict the second generation's income while controlling for the first generation's income. Since the second generation's averaged household per capita income is a single value summarizing the income of this generation across their adulthood, the dependent variable is a single continuous number. Hence, all models can be estimated using ordinary least squares regressions.

To examine the moderating role of national context on changes in average income from one generation to the next, I run all models in each country separately. I then run pooled models that interact national context with all variables to examine whether coefficient differences are statistically significant. Although the PSID and SOEP are

complex survey designs, survey weights are not employed in this analysis. Survey weights account for two major design components: oversampling of impoverished populations and initial primary sampling units. My familial controls hold constant the same factors employed in the oversampling, rendering the weights unnecessary for this component (Winship and Radbill 1994). The initial primary sampling units (counties in these surveys) only take into consideration where families lived in the initial sampling year. Since the initial surveys, families have moved and now live in five times the number of counties as the original samples. Thus, the initial primary sampling unit weights no longer reflect the geographic diversity of the data. The survey firms have addressed some of these concerns for analyses on individuals but for families—my unit of analysis—fewer adjustments are possible. Nevertheless, to take into consideration uncontrolled for biases all estimates presented utilize Huber-White robust standard errors.

1.6. Results

To begin, I empirically test whether neighborhood socioeconomic status has a larger effect on income mobility in the United States than in Germany. The baseline Model 1 in Table 1.2 indicates that it does. Specifically, results indicate that a U.S. child with parents of average income (\$13,388 per capita) who grew up in a low socioeconomic status neighborhood (defined as three standard deviations *below* the mean) would earn only 14 percent of what their parents made. In other words, they would earn an annual income of \$1,900 per capita. However, their counterpart who grew up in a high socioeconomic status neighborhood (defined as three standard deviations *above* the mean) would make

on average 290 percent of their parents' income. This represents a 37,000 dollar per capita gap. By comparison, the same calculation in Germany reveals a gap of just 19,000 dollars—approximately half that in the United States. Supplemental analyses using the pooled sample with a nation-specific interaction term affirm that this cross-national difference is statistically significant.

Next, Model 2 introduces parental controls (in addition to income). As previously discussed, much of the U.S. literature has been dedicated to differentiating structural neighborhood effects from the net impact of aggregated familial and personal characteristics. This literature finds that controlling for parental characteristics reduces observed neighborhood effects, but not entirely; substantial effects still persist (Massey, Condran and Denton 1987; Elliott et al 2006; Sampson 2012; Sharkey 2013; Chetty, Hendren and Katz 2016). Consistent with these previous studies, results here indicate the effect of neighborhood socioeconomic status in the United States is reduced by 46 percent when parental controls are added into Model 2.

Conversely, when parental controls are added into the German model, the net effect of neighborhood socioeconomic status decreases by 9.9 percent. In supplemental mediation models (available upon request), this difference is statistically insignificant, suggesting that little to no part of the German neighborhood effect is due to a “selection” effect. This finding means that observed neighborhood effects in Germany cannot be explained by underlying differences in parental factors, such as race and income. Conversely, this finding implies that the methodological conundrum regarding statistical distinction between “selection” versus “structural” neighborhood effects may largely be a U.S. phenomenon.

	Model 1			Model 2			Model 3		
	United States	Germany		United States	Germany		United States	Germany	
Neighborhood Effects									
Socioeconomic Status	6165 (590)*	3192 (380)*†		3337 (552)*	2917 (395)*		1571 (449)*	2221 (339)*	
Familial Controls									
<i>First Generation's</i>									
Per Capita Income	0.38 (0.05)*	0.37 (0.03)*		0.20 (0.05)*	0.38 (0.05)*†		0.20 (0.04)*	0.41 (0.05)*†	
Years in School				1491 (142)*	-103 (120) †		453 (117)*	-535 (115)*†	
Race (<i>Ref.—Majority Race</i>)									
Exception Race				-3085 (601)*	-3193 (1162)*		-2491 (494)*	-479 (1172)	
Other Race				-1402 (939)	-890 (633)		-980 (825)	-348 (569)	
Years Single				-1329 (1331)	-876 (1024)		148 (1101)	-2948 (954)*†	
Years with Kids				-1651 (900)	-4186 (857)*†		2816 (828)*	-617 (820) †	
Years Female Only Adults				-1006 (1224)	13 (1245)		-1278 (1101)	845 (1182)	
Workers in Household				1845 (823)*	655 (454)		-3749 (710)*	-4609 (517)*	
Age in Last Year Surveyed				38 (28)	30 (34)		48 (28)	-66 (31)*†	
Age Squared				-3 (1)*	0 (2)		-1 (1)	2 (2)	
Persons in Generation				-777 (281)*	-437 (319)		-443 (249)	-483 (281)	
Years in Survey				99 (35)*	170 (29)*		-0 (33)	79 (29)*	
<i>Second Generation's</i>									
Years in School							2186 (142)*	953 (97)*†	
Years Single							7866 (1183)*	4547 (657)*†	
Years with Kids							-13189 (809)*	-7195 (523)*†	
Years Female Only Adults							-2096 (1050)*	-2456 (899)*	
Workers in Household							12789 (690)*	7187 (370)*†	
Age in Last Year Surveyed							-79 (44)	181 (68)*†	
Age Squared							-4 (1)*	-15 (3)*†	
Persons in Generation							-3 (42)	252 (91)*†	
Years in Survey							270 (45)*	31 (76) †	
Constant	20307 (270)	16947 (188)		23236 (723)	18243 (352)		25930 (741)	19595 (485)	
R²	0.2601	0.2122		0.3279	0.2506		0.5565	0.4625	
N	2868	2214		2868	2214		2868	2214	

* Coefficient's P value ≤ 0.05 † In Pooled Estimates Germany's Coefficient is statistically distinguishable from the United States with a P value ≤ 0

Table 1.2 – Coefficients from Regression Predicting Income Mobility Using Neighborhood Socioeconomic Status.

In addition, supplemental analyses utilizing the pooled, cross-national sample indicate that the different neighborhood effects in the United States and Germany observed in Model 2 are not statistically distinguishable. In fact results indicate that, in both countries, someone who grew up in a low socioeconomic status neighborhood (three standard deviations below the mean) and whose parents made an average income would make roughly the same as their parents, yet their counterpart who grew up in a high socioeconomic neighborhood (three standard deviations above the mean) would make on average 250 percent of their parents' income. This represents a gap of 18,000 dollars per capita, holding all parental characteristics constant. However, this model does not control for individual characteristics and their impact on mobility. Model 3 of Table 1.2 introduces these second generation characteristics. As expected, in both nations the second generation's educational attainment and family structure have significant effects on income mobility. Yet, results also indicate that in both countries the direct effect of neighborhood socioeconomic status on income mobility persists and is statistically indistinguishable between the two countries.

In summary, then, direct empirical tests find that neighborhood socioeconomic status exerts a greater effect on intergenerational mobility in the United States than in Germany. However, once we control for the "selection," or matching, of families to neighborhoods along socioeconomic lines, results show that Germany and the U.S. have statistically indistinguishable neighborhood effects on income mobility. This finding is both novel and illuminating. In this specific case, national differences in neighborhood effects seem to be largely due to higher levels of socioeconomic segregation in the United States. At the same time, however, segregation levels alone do not explain away the

influence that neighborhoods have on income mobility. Indeed, in both countries the structural effects of neighborhoods on income mobility persist. This similarity leads us to question whether other neighborhood factors might also be at work, either behind the scenes of neighborhood socioeconomic status or in addition to them, as well as how the answer might vary according to national context. One contender for consideration, as discussed, is institutional resources, which European scholars commonly raise as being very differently distributed in the United States than in social democratic societies such as Germany.

1.6.1. Effects of Neighborhood Institutional Resources on Mobility

Previous U.S. and European research routinely invokes a lack of neighborhood institutional resources to explain diminished intergenerational economic mobility in and from marginalized communities. But unlike neighborhood socioeconomic status, investigators rarely operationalize institutional resources, and thus their effects remain more presumed than demonstrated. This presumption not only lends itself to empirical critique, it also raises questions about how observed effects of neighborhood socioeconomic status might change if, in fact, institutional resources were measured and considered simultaneously. The present section explores these issues. To do so, as discussed above, I created a factor score for neighborhood institutional resources that includes theoretically relevant dimensions that are comparable, if not identically measured, across both countries.

I now estimate a baseline model predicting income mobility that includes factor scores for both neighborhood socioeconomic status and institutional resources (see Model 1 of Table 1.3). As expected, introducing neighborhood institutional resources slightly

reduces the effect of neighborhood socioeconomic status in the United States. This reduction suggests that for some neighborhoods, low socioeconomic status means also having limited institutional resources. Yet, the substantial effects of both factors indicate that each has a distinct, reinforcing influence on income mobility. By contrast, neighborhood institutional resources appear to have no significant impact on income mobility in Germany.

As above, the next model adds parental controls and once again finds that they mediate the relationship between U.S. neighborhood socioeconomic status and income mobility (Model 2 of Table 1.3). Controlling for these parental factors eliminates the statistical distinction between the U.S. and German neighborhood socioeconomic status coefficients. However, when it comes to neighborhood institutional resources, parental controls are not strong mediators in either country. Thus, the difference between the countries' neighborhood institutional resources coefficients remains notable even after parental factors are taken into consideration. Indeed, even when the second generation's characteristics are controlled in Model 3 of Table 1.3, these observed differences persist. Specifically, results indicate that in Germany, neighborhood institutional resources have no effect on income mobility. By contrast, findings indicate that in the United States, if someone grew up in a neighborhood with few institutional resources (defined as three standard deviations *below* the mean) and parents with average incomes, they would make only 162 percent of their parents' income. By contrast, their counterparts who grew up in a neighborhood with ample institutional resources (defined as three standard deviations *above* the mean) would make on average 225 percent of their parents incomes. This represents a gap of 13,000 dollars per capita, net of other factors.

	Model 1		Model 2		Model 3	
	United States	Germany	United States	Germany	United States	Germany
Neighborhood Effects						
Socioeconomic Status	5649 (535)*	3189 (381)*†	3110 (527)*	2917 (395)*	1395 (430)*	2220 (339)*
Institutional Resources	3701 (683)*	-67 (198) †	2590 (577)* †	-15 (200) †	2108 (413)*	-58 (161) †
Constant	20480 (273)	16949 (188)	23244 (719)	18243 (352)	25928 (742)	19594 (486)
R²	0.2699	0.2122	0.3326	0.2506	0.5596	0.4625
N	2868	2214	2868	2214	2868	2214
* Coefficient's P value ≤ 0.05 † In Pooled Estimates Germany's Coefficient is statistically distinguishable from the United States with a P value ≤ 0.05						
Note—Model One includes partial income per capita, Model Two includes all first generation controls and Model Three adds second generation controls.						

Table 1.3 – Neighborhood Coefficients from Regressions Predicting Income Mobility with Institutional Resources.

These findings support European scholars' conjecture that the federal centralization of governmental services such as education minimizes the effect neighborhood institutional resources have on residents' income mobility. Inversely, the decentralization of governmental services to local governments and agencies in the United States amplifies the importance of neighborhood institutional resources on residents' income mobility. This support, however, should be interpreted cautiously and more as motivation for ongoing research than the final word. As mentioned above, one reason for such caution is the low Cronbach alphas associated with the factor scores for neighborhood institutional resources. These low alphas indicate that the three variables used to compute this factor, though theoretically justified, are not highly correlated empirically, which suggests some statistical inconsistency, especially in the United States. Thus, it is no surprise that in the models the U.S. standard errors are considerably higher than those for Germany. Yet, even with these larger standard errors, the models indicate a significant effect of institutional resources on income mobility in the United States but not in Germany. This pattern is the opposite of what we would expect if results were driven by measurement reliability issues alone, and not real cross-national difference in how neighborhood institutional resources work in the United States relative to Germany.

	Model 1	Model 2	Model 3	Model 4	Model 5
Neighborhood Effects					
School Expenditures Per Pupil	512 (321)			1769 (387)*	1514 (267)*
Establishments Per Residents		651 (721)		356 (642)	275 (315)
Proportion of Households with Vehicles			2816 (344)*	3308 (376)*	226 (334)
Constant	19842 (267)	19841 (267)	19871 (265)	19831 (263)	26021 (812)
R²	0.2060	0.2057	0.2301	0.2369	0.5573
N	2868	2868	2868	2868	2868
* Coefficient's P value ≤ 0.05					
Note—Models 1-4 include parental income but no other controls. Model 5 includes all first and second generation control variables.					

Table 1.4 – Neighborhood Coefficients from Regression Predicting United States' Income Mobility with Decomposed Institutional Resources Factor.

Given the strength of institutional resources in the United States, it is useful to investigate this factor further. Thus, in Table 1.4, I decompose the U.S. resource factor, examining each of the institutional resource variables separately. The first three models of Table 1.4 examine the role of each institutional resource variable on the second generation's adult income while controlling for parental income. As expected, each variable is positively correlated with intergenerational income mobility, yet not all estimated coefficients are statistically significant (at the 0.05-level). In Model 4 of Table 1.4, I include all three variables in one model, and once again all are positively correlated with intergenerational mobility, although business establishments are not statistically significant. Finally, all three institutional resource variables, as well as all first and second generational controls, are included in Model 5. Once again, results show that all indicators of neighborhood institutional resources are positively associated with intergenerational mobility, although educational expenditures are the only statistically significant factor. These supplemental models are preliminary, and caution should be taken when interpreting the relative strength of these three institutional resources on intergenerational mobility. Nonetheless, taken together these additional models build confidence in the finding that U.S. neighborhood institutional resources positively influence intergenerational income mobility, holding all else constant.

1.7. Conclusion

In attempts to comprehend and curtail persistent, intergenerational hardship, U.S. scholars have long sought to illuminate the role neighborhood environments play in the

transmission of socioeconomic status across generations (DuBois 1996[1899]; Massey, Condran and Denton 1987; Wilson 1987; Sampson 2012; Sharkey 2013; Chetty, Hendren and Katz 2016). The present study takes a step back from these efforts to assess whether the U.S. neighborhood effect on intergenerational economic mobility is a product of its unique national context. Empirically confirming what scholars have heretofore only conjectured (Veldboer, Kleinhans and Duyvendak 2002; Musterd 2005), findings indicate that neighborhoods have a stronger influence on intergenerational economic mobility in the United States than elsewhere, including specifically, Germany. Findings also suggest this national difference exists for two primary reasons.

First, residential segregation along the lines of familial socioeconomic status is higher in the United States than in Germany. This stronger “selection” effect concentrates disadvantaged families in socioeconomically disadvantaged neighborhoods, with deleterious effects. Second, a lack of neighborhood institutional resources appears to exert a stronger influence on U.S. than German intergenerational economic mobility. Together, these findings suggest that particularly high levels of residential segregation conjoin with unequal allocations of institutional resources (e.g. education and infrastructure) to contribute to larger observed neighborhood effects in the United States.

Broadly speaking, these results contradict the long-held assumption in the literature that concentrations of impoverished residents and ethnic enclaves implicitly constrain intergenerational economic mobility (Veldboer, Kleinhans and Duyvendak 2002; Musterd 2005). Although other scholars have found little to no neighborhood effects on intergenerational economic mobility in Europe (Maloutas and Karadimitriou 2001; Musterd and Deurloo 2002; Brannstrom 2004; Arbaci and Malheiros 2010; Bolt,

Phillips and Van Kempen 2010; Phillips 2010; Weeks et al. 2010; Andreotti, Gales and Fuentes 2013), this research is the first to empirically test the difference between the U.S. and a European country. The differences in neighborhood effects found in the present research show that neighborhood effects are produced in conjunction with their larger political systems. In other words, the sociopolitical environments of countries shape the role neighborhoods play in residents' housing selection and access to public resources. In turn, these differences determine how strong neighborhood effects are on intergenerational economic mobility. Thus, addressing neighborhood inequalities might not only require increasing the resources available in particular neighborhoods but also assessing the very role that neighborhoods play in the broader, sociopolitical environment.

In addition, analyses advanced here also illuminate the distinct influence of neighborhood socioeconomic status and institutional resources. In particular, results suggest that equitable distribution of formal resources does not eliminate the effect neighborhood socioeconomic status has on residents. Although the present research does not empirically test the mechanisms creating this neighborhood socioeconomic status effect, it is likely this effect is the product of informal resources shared through local networks. Thus, equal economic opportunities across all residents likely requires both access to institutional and informal resources.

1.7.1. Limitations

Even with these novel contributions, the current study is not without limitations. First, the study deploys two relative measures of neighborhood effects to make a cross-national comparison. This approach illuminates the effect of relative (dis)advantage, but it does not enable us to determine whether differences in absolute amounts of resources have distinct influences on residents. Additionally, the study was limited to the available data on neighborhood institutional resources and socioeconomic status. Future research should examine additional proxies for these factors and use measurement invariance testing to examine the comparability of various neighborhood indicators in multiple countries. In particular, future studies should examine whether the margin of errors associated with neighborhood estimates bias results and whether appropriate measures differ by the unit of analysis (Welzel and Inglehart 2016).

Second, I use Germany and the United States as two ideal types with contrasting approaches to the distribution of resources and conceptions of housing and community. Yet, it remains unclear whether the observed patterns are emblematic of these two ideal types or simply particularities of these two nations. Third, while differences between the two nations enable me to infer whether general policies and practices assist in the production of neighborhood effects, I do not test specific policies. Hence I can say little about which specific policies shape neighborhood effects.

1.7.2. Moving Forward: Implications for Research and Policy

Caveats aside, results of the present study encourage a shift in perspective. Currently, in the United States, policies focus on increasing the resources of specific neighborhoods through federally or privately funded development projects. Findings here suggest that even though increases in individual neighborhood resources can be helpful, national interventions might be more influential by broadly adjusting the housing market and federal redistribution system. U.S. scholars and practitioners often conceptualize U.S. segregation as primarily a product of a racial history and personal preferences. Although these factors are certainly substantial, the comparison with Germany suggests future research should look into how contemporary federal policies and housing markets contribute to segregation levels. Moreover, based on German policies and practices, the most fruitful means might include centralizing the distribution of resources so that all communities are seen as deserving of quality public services instead of commodities which vary in quality based on residents' purchasing power. The underlying point is that adjusting the very role of the neighborhood in residents' lives might be more effective in reducing intergenerational economic hardships than directly targeting underprivileged neighborhoods or relocating marginalized populations, though the two strategies might be usefully combined.

Additionally, in Germany and across Europe, conventional wisdom presumes that if resources are equally distributed, residential segregation will not negatively influence residents. Contrary to this perspective, the present study suggests that socioeconomic integration is also likely to be essential for decreasing the intergenerational transmission of economic status. This finding means that in both the United States and Germany,

instead of thinking of disadvantaged neighborhoods as the only ones that need “fixing,” fostering economic opportunities for all residents will require ensuring all neighborhoods are welcoming to all residents.

Moving forward, researchers and practitioners in multiple national contexts should continue to consider the influence that childhood neighborhoods can have on economic opportunities throughout the life course. Yet, these relationships should not be considered in a vacuum. Instead, broader city, national and even global factors creating the meanings behind and resources associated with respective neighborhood spaces should be taken into account. By doing so, we can continue to reimagine how all spaces can better provide economic opportunities for all residents.

Chapter 2

The Truly Advantaged: Re-conceptualizing the Implicit Neighborhood of Neighborhood Effects

2.1. Executive Summary

Urban sociologists have long studied U.S. neighborhood inequality, but the focus on neighborhoods was amplified after William J. Wilson's *The Truly Disadvantaged* argued that neighborhoods affect life chances. The ensuing proliferation of 'neighborhood effects' studies has focused primarily on marginalized communities rather than the full spectrum of U.S. neighborhoods. The present study utilizes the geocoded Panel Study of Income Dynamics to examine the influence of childhood neighborhoods on educational attainment. The findings suggest that the structural influences of neighborhoods, above and beyond familial or individual characteristics, is strongest not in marginalized

neighborhoods but in advantaged neighborhoods. The paper concludes with a discussion of findings and argues that, in both academic research and policy creation, addressing the issues associated with the Truly Disadvantaged requires examining the compounding privilege of the Truly Advantaged.

2.2. Introduction

U.S. urban sociologists have long noted the unequal distribution of resources and residents across neighborhoods (e.g. DuBois 1996 [1899]; Park and Burgess [1925]1967; Johnson 1943; Wilson 1987; Massey and Denton 1993; Sampson 2012). Yet, this observation gained increased attention across the discipline after the publication of William J. Wilson's (1987) *The Truly Disadvantaged* (Small and Newman 2001). Wilson's claim, that the limited institutional resources, job opportunities, human capital, social networks and residential stability in Black inner-city neighborhoods restricted residents' socioeconomic opportunities, inspired a multitude of qualitative and quantitative studies investigating the influence of neighborhoods on life chances (e.g. Brooks-Gunn et al. 1993; LeClere, Rogers and Peters 1997; Bourgois 2002; Reijneveld 2002; Turley 2003; Venkatesh 2006; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Kimbro, Denney and Panchang 2012; Martens et al 2014).

These studies—commonly referred to as neighborhood effects studies—have varied widely in regards to their outcome of interest, methodological approach, and empirical conclusions. Yet, they have almost exclusively focused on disadvantaged neighborhoods (Johnson 2013). Put another way, few studies have examined or discussed

the implications of neighborhood effects in average or advantaged neighborhoods. This focus on disadvantaged neighborhoods would be understandable if empirical evidence suggested only disadvantaged neighborhoods influence residents while advantaged neighborhoods have no influence on residents' outcomes. However, initial investigations comparing the effects of disadvantaged compared to advantaged neighborhoods suggest the opposite. High socioeconomic status neighborhoods have stronger effects on their residents than low socioeconomic status neighborhoods (Johnson 2013). This finding questions the scholarly conception of neighborhood effects. Nevertheless, it has not been established with more traditional measures of neighborhood disadvantage nor differentiated by residents' race.

Utilizing the restricted geocoded Panel Study of Income Dynamics, I examine whether traditional measures of neighborhood disadvantage have a curvilinear relationship with educational attainment and whether this relationship differs by residents' racial identification. I conclude by discussing the theoretical and policy implications of these findings.

2.3. Neighborhood Effects Literature

Over the last 25 years, the neighborhood effects literature has produced a plethora of studies examining the extent to which neighborhoods have lasting effects on their residents' wellbeing. These studies have explored whether neighborhoods influence residents' socioeconomic status, health, and civil engagement using: quasi-experimental designs, geocoded, longitudinal panel data, ethnographic observations, and interviews

(e.g. Brooks-Gunn et al. 1993; LeClere, Rogers and Peters 1997; Bourgois 2002; Turley 2003; Stewart, Stewart and Simons 2007; Ainsworth 2010; Mahatmya and Lohman 2012; Massey et al 2013; Martens et al 2014; Chetty, Hendren and Katz 2016). Despite some variation in magnitude, on the whole, scholars conclude neighborhood contexts influence residents' outcomes, especially when multigenerational effects are taken into consideration (Entwisle, Alexander, and Olson 2005; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Sampson 2012; Sharkey 2013).

Theoretically, scholars explain this relationship with three rationales: socialization within disadvantaged neighborhoods discourages academic success and participation in the formal economy; the lack of institutional resources limits opportunity; and a void of collective efficiency precludes collective action (Small and Newman 2001). For example, when examining educational attainment—a key variable because of its direct relationship with neighborhood context and its influence on other socioeconomic and health outcomes—researchers discuss how social norms in neighborhoods shape how much time students expect to allocate to homework, which in turn influences their educational success (Ainsworth 2002; Ainsworth 2010; Casciano and Massey 2012; Brattbakk and Wessel 2013). Additionally, scholars discuss how the social expectations, networks and formal resources in neighborhoods influence students' educational aspirations and their ability to achieve these objectives (Ainsworth 2002; Andersson and Subramanian 2006; Brattbakk and Wessel 2013).

Although these theoretical explanations apply to both the lack of resources in disadvantaged neighborhoods and the existence of these resources in advantaged neighborhoods, scholars have disproportionately focused on disadvantaged communities.

The emphasis on inner-city poverty has been essential for illuminating unjust conditions and inspiring thousands of governmental policy adjustments as well as nongovernmental programmatic interventions. These scholarly findings have inspired two types of neighborhood interventions: the decentralization of public housing through housing vouchers, and the funding of community development organizations in impoverished communities such as the Harlem Children's Zone (Duneier 2016). Nevertheless, this focus on disadvantaged neighborhoods has simultaneously limited the understanding of the role middle and upper income neighborhoods have on their well-off and White residents (Johnson 2013) and unintentionally exoticized ghetto poverty (Small 2015).

Noting that little research has explicitly examined whether neighborhood effects vary across neighborhood types, Johnson (2013) conducted a meta-analysis on 84 preexisting neighborhood effect studies. Like the literature more broadly, these studies primarily discussed disadvantaged neighborhoods in their theoretical framing and conclusions. However, he selected studies whose samples included residents across all neighborhood types. Johnson demonstrated that, when the coefficients from these studies were differentiated between high and low socioeconomic status neighborhoods, the high socioeconomic status neighborhoods had greater effects on their residents than lower socioeconomic status neighborhoods. His findings provide initial evidence that neighborhoods matter across various neighborhood types. Nevertheless, being a meta-analysis, these results have two important constraints.

First, to ensure comparability across the studies, Johnson restricted the sample to studies that utilized neighborhood socioeconomic status as their neighborhood indicator. In other words, he excluded studies whose neighborhood indicators included other

demographic factors such as the proportion of single parent families or Black residents. Given that Johnson's goal was to compare across studies, he needed to exclude studies that utilized disadvantaged indexes because these indexes are measured inconsistently. However, as Johnson acknowledges, excluding these commonly utilized measures might skew his results. Second, Johnson was unable to examine whether his findings held for both White and Black residents. Given the racialized history of the United States, the majority of U.S. disadvantaged neighborhoods are overwhelmingly Black and there are no comparable concentrations of impoverished Whites (Massey and Denton 1993). Thus, Johnson's results could be reflecting the impact neighborhoods have on Whites who do not live in the most disadvantaged communities. Put another way, his results could miss the real and substantial impact extremely disadvantaged neighborhoods have on their Black residents.

The present study examines whether Johnson's findings hold even when neighborhood disadvantage indexes include the proportion of single-parent families and Black respondents in the neighborhood, and when interactions between racial identity and neighborhood effects are examined. Specifically, I examine whether neighborhood effects on educational attainment are comparable across all neighborhoods and across racial groups. Additionally, building off the findings that neighborhood effects are strongest when measured across time (Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011), this study utilizes neighborhood data across respondents' entire childhoods (ages zero to 18) to capture robust estimates of neighborhood effects across all U.S. neighborhoods.

2.4. Data and Methods

Previous quantitative studies examining childhood neighborhood effects on adult outcomes have used one of two methods: quasi-experimental designs or geocoded, longitudinal panel data. Quasi-experimental designs compare low-income residents who were and were not relocated to higher-income communities as part of the Gautreaux Project or the Moving to Opportunity Experiment. This method eliminates some neighborhood selection bias, arguably enabling research to illuminate the impact of neighborhoods void of familial neighborhood selection processes (Fautha, Leventhal and Brooks-Gunn 2005; Casciano and Massey 2012; Massey et al 2013; Chetty, Hendren and Katz 2016). Nevertheless, the restriction of such data to low-income families who originated in disadvantaged neighborhoods means such data could not be utilized for the purposes of this study. Thus, I utilize the second and more common methodological neighborhood effects design—geocoded, longitudinal data—to examine poor and non-poor families across their children’s entire childhood (e.g. Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011).

The Panel Study of Income Dynamics (PSID) is the longest running national representative geocoded data set. Since 1968, the PSID has collected annual data on employment, wages, income, education, expenditures and wealth. Following the initially surveyed families, their children, and their children’s children, the sample now includes 9,000 households and over 22,000 individuals. Given the study design, some of these individuals have annual data points from their birth through their early 40s while others have data from mid-life to their death. This variety means the PSID can be used to

answer a wide range of questions. Yet, it also means researchers must decide which individuals in the sample are most advantageous for a given research question.

The present study is interested in how childhood neighborhoods influence educational attainment. Thus, I restrict the sample to individuals born within a ten year period—1975 and 1985—*and* who were still in the survey at age 26. With these conditions, the sample has 2,367 individuals. The vast majority (94 percent) of these individuals identify as either White or Black—reflecting the U.S. racial dynamics in 1968 when the initial sample was selected.¹⁴ However, this means that too few individuals identify as each of the other racial categories—Hispanic, Asian, Native American—to analyze each of these categories separately. Since the experiences of these groups are distinct from one another and from Whites and Blacks, I followed the precedent in the literature and exclude these 140 individuals (Sharkey 2013).

2.4.1. Educational Attainment

As previously noted, the neighborhood effects literature includes a wide range of outcome variables. For this investigation, I take the lead of Wilson (1987) and the majority of the neighborhood effects literature and focus on socioeconomic status. Common operationalizations of socioeconomic status include income and education. I

¹⁴ In the late 1990s, the PSID added immigrant families to reflect the increase in immigration since 1968. However, these added families do not yet have enough data to meet the conditions of this study. Additionally, given the findings of Howell and Emerson (2016), I operationalize race as five monoracial categories—categorizing all multiracial individuals as the racial group lower on the ethnoracial hierarchy.

choose to focus on education due to the age of my cohort. That is, the youngest respondents in my sample are only 26 years old at the end of my observation period. Since the majority of individuals have completed their education by this age, educational attainment at age 26 can be seen as a reliable measure of their educational status. However, incomes at age 26 vary by profession. For example, at age 26, doctors are still in residency. Thus, their education reflects their socioeconomic status but their income does not yet reflect their high socioeconomic status. Hence, by focusing on educational attainment, I am better able to estimate the long term socioeconomic status of these respondents.

Educational attainment is operationalized as completed years in school. The variable ranges from 5 to 17. In other words, all respondents with more than a bachelor's degree are given a 17 for completed years in school. The upper end censor was created by PSID but is also advantageous for my purposes. Since education has a lower bound, the right tale skew of the upper end could exacerbate the impact of neighborhood on high socioeconomic status individuals. This upper censor ensures the influences of neighborhoods are not exaggerated by the distribution of the dependent variable. Additionally, to test the robustness of the models, I run models with education operationalized as categorical (i.e. less than high school, high school diploma, some college, bachelor's degree, and graduate school). All substantive results were comparable to the findings presented and are available upon request.

2.4.2. Neighborhood Disadvantage Index

Reflecting the focus in the literature on disadvantaged neighborhoods, neighborhood factors are often called disadvantaged indexes and include variables such as the proportion of the neighborhood that is Black, living in poverty, and single parents. Although these are all “disadvantaged” statuses, these variables have a full range of values. In other words, all neighborhoods—including White upper-class neighborhoods—are given disadvantaged index scores. In extremely advantaged neighborhoods these values are just very low. For congruency with previous literature, I utilize a neighborhood disadvantaged index but highlight that low values of neighborhood disadvantaged denote advantaged neighborhoods.

The neighborhood disadvantage factor was derived using the 1980, 1990 and 2000 Census Long Form as well as the 2005-2010 American Community Survey. I use census tracts as a proxy for neighborhoods. To have consistent boundaries across time, I normalized all tracts to the 2010 census tracts (Logan, Xu and Stults 2014). Using these boundaries, I conducted a factor analysis. Based on previous research, twenty neighborhood characteristics were considered in the initial analyses (e.g. median income, proportion with at least a bachelor’s degree, proportion with at least a high school diploma, mobility rate, owner occupancy rate, unemployment rate, and average room per capita). From this analysis, I concluded that disadvantage could be captured with one factor that included poverty rate, the proportion of the census tract’s households with children that were headed by single parents and the proportion of the census tract that was Non-Hispanic Black. A standardized factor was derived using all census tracts in the country for each decade. Given that the index is standardized, scores of zero denote

average U.S. neighborhoods. For descriptive purposes, I discuss all neighborhoods with neighborhood disadvantage scores below zero as advantaged neighborhoods and those with scores above zero as disadvantaged neighborhoods.

After the neighborhood disadvantage score was calculated for each decade, I utilized linear imputation to estimate the neighborhood disadvantage of each census tract in every year. These yearly neighborhood disadvantage factors were then linked to PSID respondents' addresses. This means respondents' neighborhood disadvantage scores can vary annually because of demographic changes in their communities *or* residential moves between neighborhoods. As previous research has demonstrated, the duration of exposure to particular neighborhood type is an essential factor impacting socioeconomic trajectories (Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Sharkey 2013; Chetty, Hendren and Katz 2016). Thus, unlike in the majority of studies in Johnson's (2013) meta-analysis, I take advantage of the richness of the PSID data and conceptualize childhood neighborhoods as a composite score of one's childhood neighborhoods.¹⁵ To create the composite childhood neighborhood disadvantage score, I calculated the mean of the annual neighborhood disadvantage scores across all years during which the respondent was under 18 and living with their parents.

¹⁵ Given that not all places were assigned census tracts or BNAs (rural tracts) in the 1970 Census, I begin the neighborhood disadvantage in 1980. Since the sample was born between 1975 and 1985, this means for some individuals the neighborhoods that they lived in during the first few years of their childhood were not included in the averages.

2.4.3. Controls

As widely noted, the same socioeconomic and demographic factors that contribute to families' neighborhood selection correlate with their children's educational attainment (Turley 2003; Sharkey and Elwert 2011). Thus, neighborhood attributes are in part capturing an aggregated effect of family socioeconomic status. In order to approximate the impact of neighborhood exposure as distinct from family level features, it is common practice to control for familial socioeconomic and demographic factors.

All models include two respondent level controls and five family level controls. The individual controls are race and gender. As discussed previously, I operationalized respondents' racial identity as White or Black and respondents' gender as female or male.¹⁶ Familial controls are calculated for the time the respondent was living in their parents' home and under 18 years of age and include parental income, education, marital status, number of siblings, and number of moves.¹⁷ Parental income is the average household income across the respondents' childhood. Specifically, for every year, I summed the income of the parents present in the household and converted this household income to 2010 dollars. Then the mean income was calculated across all years the respondent was under 18 and lived with their parents.¹⁸ Similarly, parental education also

¹⁶ PSID to date has only allowed respondents to choose from binary gender categories.

¹⁷ All models were also run with regional controls. However, region did not play any substantive or statistical significance and thus were not included in the final models.

¹⁸ While the exact dollar amounts are displayed in the descriptive tables, for the models, income was divided by 10,000 so that coefficients were easier to interpret.

contains a temporal component. I compare each year of father's and mother's years of education completed, taking the highest attainment as the familial attainment. I then averaged the yearly educational attainment across all years.¹⁹ Parental marital status is operationalized as the proportion of years the parents were married during the respondents' childhood. Number of siblings is the average number of children living in the household across respondents' childhood. Finally, number of moves is the number of times the respondent moved during their childhood.

Research has shown that controlling for parental socioeconomic status produces a conservative estimate of neighborhood effects because parent's past neighborhood locations influence their socioeconomic status, which in turn influences their neighborhood attainment (Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011). However, given that my design examines childhood neighborhoods' influence on educational attainment in order to adopt a more robust multigenerational approach, I would need the neighborhood attributes of parents' childhood neighborhoods. To date, no longitudinal data includes complete data on childhood neighborhoods for two adult generations. Hence, all estimates derived in this research are conservative because they

¹⁹ Alternative approaches include parental educational attainment at one point in time or the highest educational attainment in the entire time period. These approaches, however, lose some of the complexity available in the data. For example, consider a hypothetical family whose father is college educated but whose mother has a high school diploma. The father dies when the child is 5. While this child will benefit from having a father who was college educated, they were primarily raised by their high school educated mother. Thus, their educational attainment will likely reflect this fact. The temporal approach enables us to capture this complexity.

do not take into account how neighborhoods have also influenced the familial socioeconomic statuses controlled for in the models.

2.4.4. Statistical Modeling

Given that the dependent variable—educational attainment—is continuous, I utilized ordinary least squares estimation to examine the role that neighborhoods have on educational attainment. However, since multiple respondents grew up in the same families, I use multilevel modeling to account for multiple siblings within one family. Specifically, all models presented in the paper were estimated using Stata's `xtreg` command. To examine whether neighborhood effects have a nonlinear relationship with educational attainment and whether neighborhood effects differ by residents' race, I introduce interactions into the models.

2.5. Results

To examine descriptive differences between advantaged and disadvantaged neighborhoods, I divide the sample into two groups: respondents with advantaged childhood neighborhoods and those with disadvantaged childhood neighborhoods. As mentioned above, I define advantaged childhood neighborhoods as those with below average neighborhood disadvantage (below zero) and disadvantage childhood neighborhoods as those with above average neighborhood disadvantage (above zero). To

clarify, this means that “advantaged” neighborhoods include both middle-class and upper-middle-class communities.

In this binary conception of neighborhoods, disadvantaged neighborhoods have disadvantage scores ranging from 0 to 6.89 with a mean of 1.53. To help contextualize these standardized factor scores, consider that respondents with a neighborhood disadvantage score of zero lived in neighborhoods that were on average 13 percent poor, 9 percent single-parent families, and 10 percent Black. Respondents with a neighborhood disadvantage score of one lived in neighborhoods that were approximately 26 percent poor, 12 percent single-parent families and 32 percent Black. Across all the neighborhoods categorized as ‘disadvantaged’ (above zero on the neighborhood disadvantage scale), the mean percent poor is 24, mean Black proportion is 52 and mean single-parent proportion is 18.²⁰

²⁰ To be clear, neighborhoods can have differing values on individual variables and the same neighborhood disadvantaged factor because they are composite scores. Thus, the proportions provided here are simply the mean values across all neighborhoods with a particular factor score.

	Disadvantaged	Advantaged
Dependent Variables		
Completed Years in School at Age 26	12.94 (2.01)*	14.15 (2.00)*
Neighborhood Factor		
Disadvantage Index	1.53 (1.14)*	-0.44 (0.24)*
Individual Demographics		
White	0.20 (0.40)*	0.96 (0.20)*
Female	0.54 (0.50)*	0.50 (0.50)*
Childhood Parental Controls		
Parents' Income	38,002 (31,147)*	79,497 (55,504)*
Parents' Years in School	12.27 (2.12)*	14.06 (2.16)*
Proportion of Years Parents Married	0.59 (0.40)*	0.88 (0.22)*
Number of Siblings	2.48 (0.99)*	2.26 (0.77)*
Number of Moves	3.38 (2.90)*	2.31 (2.30)*
N—Individuals (Families)	1087 (502)	1140 (545)

*Denotes the two-sided t-test comparing the mean of Disadvantaged and Advantaged Neighborhoods has a P-Value ≤ 0.05 .

Table 2.1 – Descriptive Statistics for Disadvantaged and Advantaged Neighborhoods.

Conversely, advantaged neighborhoods in this sample have neighborhood disadvantage scores ranging from -1 to 0 with a mean value of -0.44. While the sample has comparable numbers of respondents from advantaged and disadvantaged neighborhoods, advantaged neighborhoods are more homogeneous than disadvantaged neighborhoods—as evidenced by their smaller standard deviation and range (see Table 2.1). This pattern is expected, given U.S. neighborhood disadvantage more generally. Yet, these neighborhoods still vary from those with a score of -1 that on average have 2 percent poverty, 3 percent single parent families and 1 percent Black residents, to those with a score of zero which, as mentioned previously, have on average 13 percent poverty, 9 percent single parent families, and 10 percent Black residents.

As expected given previous research on neighborhood effects, in my sample, those who grew up in the disadvantaged neighborhoods have less education than those who grew up in advantaged neighborhoods—12.95 years compared to 14.15. Yet, as seen in Table 2.1, individuals who grew up in disadvantaged neighborhoods are also more likely to be Black, female, and raised in families with less income, less education, less marriage, more siblings, and more residential moves. All of these controls also correlate with educational attainment. Thus, differences in educational attainment across the two groups might be due to these covariates and not to the neighborhoods themselves. Hence, I now turn to the multiple regression analyses to examine whether neighborhoods still influence educational attainment when individual and family factors are held constant.

Since I am specifically interested in whether neighborhood effects are stronger for disadvantaged compared to advantaged neighborhoods, I first run stratified models—examining each type of neighborhood separately. As shown in Table 2.2, when holding individual and family factors constant, neighborhood disadvantage does not influence residents’ educational attainment in disadvantaged neighborhoods. To contextualize this finding, consider a child who grew up in a neighborhood with approximately 13 percent poverty, 9 percent single parent families, and 10 percent Black residents. Results indicate that this child completed the same amount of education as a child who grew up in a neighborhood that was 60 percent poor, 57 percent single parent, and 89 percent Black.²¹ In short, neighborhoods do not influence educational attainment in the lower half of the

²¹ This is the average proportion for neighborhoods with a neighborhood disadvantage score greater than 4.5.

neighborhood disadvantage distribution. However, neighborhoods do impact residents in more advantaged neighborhoods.

	Disadvantaged	Advantaged
Neighborhood Factor		
Disadvantage Index	-0.08 (0.06) [†]	-0.95 (0.25)* [†]
Individual Demographics		
White	-0.26 (0.16)	-0.20 (0.28)
Female	0.53 (0.11)*	0.65 (0.10)*
Childhood Controls		
Parents' Income	0.09 (0.03)* [†]	0.04 (0.01)* [†]
Parents' Years in School	0.26 (0.03)*	0.34 (0.03)*
Proportion of Years Parents Married	0.47 (0.18)*	0.53 (0.26)*
Number of Siblings	-0.19 (0.06)*	-0.16 (0.07)*
Number of Moves	-0.05 (0.02)*	-0.07 (0.02)*
Constant	13.39 (0.13)	13.10 (0.28)
Between R ²	0.3554	0.3263
N—Individuals(Families)	1087 (502)	1140 (545)

*Denotes the coefficient is statistically significantly different from zero with a P-Value ≤ 0.05 .

[†]Denotes Disadvantaged Neighborhood coefficient is statistically significantly different from Advantaged Neighborhood coefficient with a P-Value ≤ 0.05 .

Table 2.2 – Coefficients from Stratified Regressions Predicting Educational Attainment.

As seen in Table 2.2, residents in advantaged neighborhoods are substantively and statistically influenced by their neighborhood context. Specifically, residents in average U.S. neighborhoods complete, on average, one year less of school than residents in the most advantaged neighborhoods. Said another way, individuals who grew up in the 1980s and 1990s in neighborhoods with 2 percent poverty, 3 percent single parent families and 1 percent Black residents completed an additional year in school compared to their counterparts who grew up in neighborhoods with 13 percent poverty, 9 percent single

parent families, and 10 percent Black residents. This finding contradicts the notion that disadvantaged neighborhoods have a particularly potent impact on their residents' socioeconomic status. But it does not examine whether the difference between estimated neighborhood effects across the two models is statistically distinguishable.

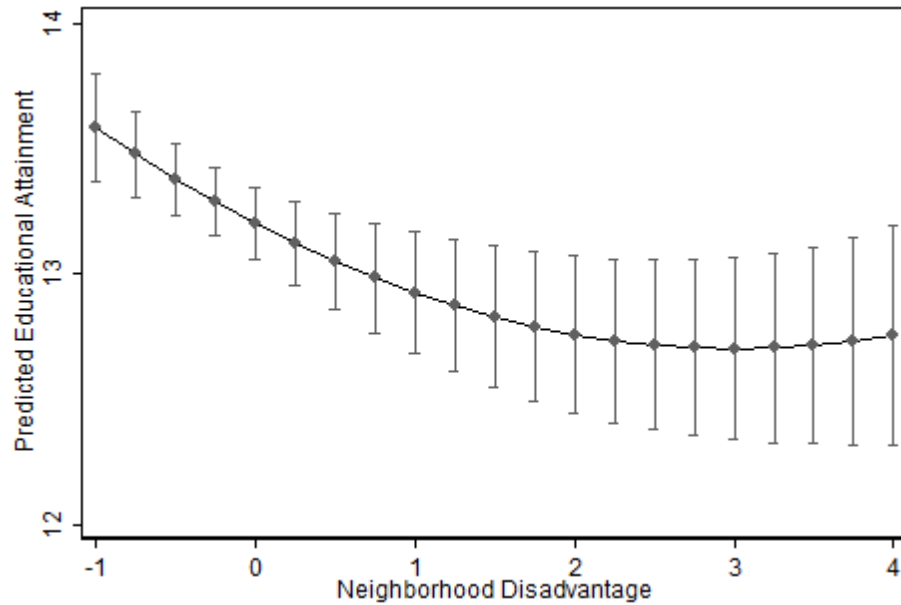
For this test, I estimated a second model with all respondents in which every independent variable was interacted with a dummy variable denoting whether one's childhood neighborhood was advantaged or disadvantaged. The interactions in this model examine whether the magnitude of the coefficients in disadvantaged neighborhoods is distinct from their corresponding coefficients in advantaged neighborhoods. As denoted in Table 2.2, the difference between the neighborhood effects is indeed significant (at the 0.05 level), suggesting it is not merely a particularity of this sample but reflective of a larger population trend. Moreover, the only other coefficient that is significantly distinct between the two models is parental income. Specifically, for those living in disadvantaged neighborhoods, every 10,000 dollar increase in household income corresponds with nearly one more year of education. Yet, for those in advantaged neighborhoods a 10,000 dollar increase in household income corresponds with only a half-year increase in education. In other words, educational attainment in disadvantaged neighborhoods is more dependent on the individual family's socioeconomic status; whereas in the advantaged neighborhoods, residents' educational attainment is more dependent on the neighborhood. This pattern gives additional evidence that the influences of neighborhoods are strongest in advantaged neighborhoods.

To ensure the large range in neighborhood disadvantage scores found in disadvantaged neighborhoods is not skewing these results, I reran these models with only

respondents who grew up in neighborhoods with a neighborhood disadvantage score below one. By doing this, both advantaged and disadvantaged neighborhood categories only included a range of one standard deviation in their neighborhood disadvantage scores. These supplemental models affirm the above findings and are available upon request.

In summary, these initial models suggest that advantaged neighborhoods have a stronger influence on residents' education than disadvantaged neighborhoods. However, this approach is unable to examine whether this finding is due to a nonlinear relationship between neighborhoods and education. To examine whether this relationship is nonlinear, I ran a model which did not include the binary neighborhood categories but introduced a quadratic neighborhood disadvantage term.²² As visualized in Figure 2.1, this model demonstrates that the impact of neighborhoods is in fact gradual. Specifically, the slope representing neighborhood influence on residents is steep for residents living in the upper thresholds of neighborhoods but minimal for residents in more disadvantaged communities—as depicted by the horizontal slope on the right side of the graph. This provides additional evidence that neighborhood effects are particularly strong in advantaged neighborhoods.

²² Before I introduced the quadratic term, I shifted the neighborhood disadvantaged index to the right such that the minimum value was zero instead of negative one. This ensures the quadratic term is distinct for the positive and negative values.



Note: For consistency with the prose, the x-axis is labeled as -1 to 4 but in the model the neighborhood disadvantaged index was shifted so that the minimum was zero.

Figure 2.1 – Predicted Educational Attainment by Neighborhood Disadvantage.

Taken together, these findings provide compelling evidence that advantaged neighborhoods drive observed neighborhood effects. However, these findings might be due to the fact that, in the United States, advantaged neighborhoods are primarily occupied by White residents. In fact, even in this sample, 96 percent of the residents in advantaged neighborhoods are White while 80 percent of residents in disadvantaged neighborhoods are Black. On the whole, Whites do not live in the inner-city, marginalized communities widely discussed in the literature and no comparable concentrations of White poverty exist (Massey and Denton 1993). Hence, these findings might simply reflect racial differences and not divergent neighborhood effects.

	Black	White
Neighborhood Factor		
Disadvantage Index ¹	-0.18 (0.19) [†]	-1.00 (0.33)* [†]
Disadvantage Index Squared ¹	0.01 (0.03) [†]	0.30 (0.14)* [†]
Individual Demographics		
Female	0.55 (0.11)*	0.62 (0.09)*
Childhood Controls		
Parents' Income	0.08 (0.03)*	0.04 (0.01)*
Parents' Years in School	0.22 (0.03)* [†]	0.36 (0.03)* [†]
Proportion of Years Parents Married	0.57 (0.19)*	0.46 (0.23)
Number of Siblings	-0.24 (0.06)*	-0.11 (0.07)
Number of Moves	-0.05 (0.02)*	-0.06 (0.02)*
Constant	13.55 (0.28)	13.80 (0.18)
Between R ²	0.3423	0.3299
N—Individuals(Families)	916 (383)	1311 (592)

*Denotes the coefficient is statistically significantly different from zero with a P-Value ≤ 0.05 .

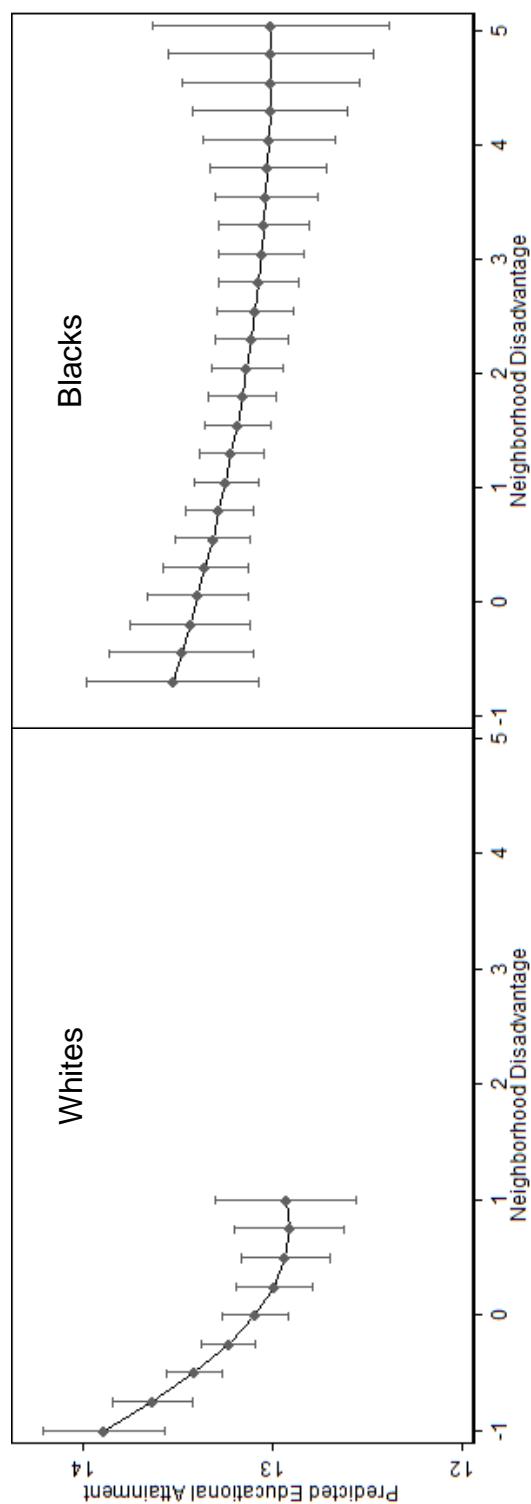
[†]Denotes Black coefficient is statistically significantly different from White coefficient with a P-Value ≤ 0.05 .

¹ For this model, the neighborhood disadvantaged index was adjusted so that the minimum was zero to ensure when squared negative values were distinct from their positive counterparts.

Table 2.3 – Coefficients from Regressions Predicting Educational Attainment Stratified by Race.

To test the possibility that the previous findings are due to racial differences, I ran stratified models for Black and White residents which include the quadratic neighborhood effects term. For Blacks, neighborhood disadvantage's linear and quadratic terms are insignificant (see Table 2.3). In other words, neighborhoods do not influence Black residents. However, neighborhoods have a potent and curvilinear influence on Whites which means that neighborhood factors influence Whites above and beyond their familial characteristics if they live in the most advantaged neighborhoods. I confirm these differences are significant by interacting racial identification with neighborhood disadvantage (both the linear and quadratic terms).

In summary, Whites are concentrated in more advantaged neighborhoods and residential location has a profound impact on their educational attainment (as visualized in Figure 2.2). Specifically, Whites living in the most advantaged neighborhoods are most strongly influenced by their neighborhood context. Blacks, on the other hand, are spread more equitably across neighborhood types although they are disproportionately concentrated in disadvantaged neighborhoods. Furthermore, neighborhoods have little influence on Blacks' educational attainment, particularly the educational attainment of Blacks in extremely disadvantaged neighborhoods compared to average neighborhoods. In short, as previous research has demonstrated, childhood neighborhoods do influence adult socioeconomic status. However, this effect comes primarily from advantaged not disadvantaged neighborhoods.



Note: For consistency with the prose, the x-axis is labeled as -1 to 4 but in the model the neighborhood disadvantaged index was shifted so that the minimum was zero.

Figure 2.2 – Predicted Educational Attainment by Neighborhood Disadvantage and Residents Race.

To further investigate the influence of advantaged neighborhoods on residents, I ran supplemental analyses using a neighborhood advantaged index. Mirroring Ainsworth (2002) and Browning et al. (2006), I created a scalar variable, which included proportion of the census tract with bachelor's degrees, proportion of the census tract in professional or managerial occupations and the proportion of household incomes above \$75,000 dollars (in 2010 dollars). As with the neighborhood disadvantaged index, results indicate that children who grow up in more advantaged communities complete more education (see Appendix A). Furthermore, this relationship between childhood neighborhoods and educational attainment is stronger at the high end of the distribution. Finally, I introduce the advantaged index into a model with the disadvantaged index.²³ Results indicate that the advantaged index is larger in magnitude and reduces the influence of the disadvantaged index to statistical insignificance. These supplemental examinations add additional evidence that advantaged communities have a particularly strong effect on residents' educational attainment.

2.6. Discussion and Conclusion

Since Wilson's (1987) *The Truly Disadvantaged* re-centered sociological attention on neighborhood inequality, the neighborhood effects literature has repeatedly demonstrated that childhood neighborhoods influence socioeconomic mobility above and beyond

²³ Although the two are correlated ($r = 0.53$), multi-collinearity is not an issue in the model.

parental and individual characteristics (Turley 2003; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Massey et al 2013; Chetty, Hendren and Katz 2016).

Specifically, this literature has focused on the detrimental impact living in disadvantaged neighborhoods can have on residents' wellbeing (Johnson 2013). Utilizing the restricted, geocoded PSID data, the present study confirms the finding that neighborhoods impact socioeconomic mobility but also demonstrates that the relationship is not driven by the influence of disadvantaged neighborhoods. Instead, advantaged children raised in neighborhoods with extremely low poverty rates, Black proportions and single-parent family percentages experience compounding privileges. Conversely, children growing up in neighborhoods with extreme poverty and high proportions of single-parent families and a large Black population achieve levels of education compare to as their counterparts in average neighborhoods.

Scholars' focus on marginalized communities has been essential for highlighting injustices and bringing needed attention to the persisting inequalities. However, this focus has ignored the empirical reality that structural effects of neighborhoods are strongest in advantaged communities. Furthermore, as critical race theorists have repeatedly demonstrated, concentrating on marginalized groups often has the unintended consequence of normalizing the numeric or politically dominant group. For example, the focus of early race scholars on Nonwhites de-emphasized the social construction of Whiteness. In turn, these studies often reinforced the patronizing beliefs about Nonwhites (Feagin 2010; McKee 1993; Turner 1978; Winant 2007). Likewise, urban theorists' focus on disadvantaged neighborhoods has unintentionally normalized middle and upper class

White neighborhoods and exotified lower class Black and Brown communities (Small 2015).

This selective focus on disadvantaged neighborhoods means scholars know considerably more about disadvantaged communities than their advantaged counterparts. Thus, it remains unclear exactly how advantaged neighborhoods enhance the socioeconomic opportunities of the advantaged. However, pulling from research on inequality more generally, I posit that the additive effect of opportunities begetting opportunities or opportunity hoarding is contributing to the observed effect of advantaged neighborhoods (Merton 1988; Tilly 1998; DiPrete and Eirich 2007; Abbott 2014). Specifically, I surmise that social and institutional connections to societies' most privileged spaces provides all residents with socioeconomic opportunity. For example, local schools in advantaged neighborhood benefit from increased public funding given the higher property taxes in these areas. On top of this increased funding, they are more likely to receive substantial donations from parents and alumni. Moreover, parents and alumni with elite positions can provide students access to employment networks. Lastly, children that grow up in these communities are expected to be successful no matter their own merit or work ethic. Thus, they are given the benefit of the doubt when applying for jobs or entrance into elite universities.

In short, more research is needed to create an explicit and robust theory of advantaged neighborhoods and how they facilitate the intergenerational transmission of socioeconomic privilege. Conducting research on these mechanisms will enable researchers to propose new tax benefits, federal policies, and neighborhood programs that can ensure advantage is equitably distributed and not unduly concentrated. Nevertheless,

even without future studies on advantaged neighborhoods, this research demonstrates a need for researchers and practitioners alike to reframe our conversations about neighborhoods.

Reframing the neighborhood effects conversation to encompass the influences of advantaged neighborhoods on residents' socioeconomic mobility does not diminish the very real and detrimental socioeconomic and physical consequences Black and poor families in impoverished Black neighborhoods endure. In fact, the results of this research reaffirm that socioeconomic opportunities are unequally distributed across United States residents, which implicitly has negative implications for the most disadvantaged. Nonetheless, similar to Johnson's (2013) meta-analysis, these findings suggest that to address neighborhood inequality, research and policy intervention should consider not only what is lacking from disadvantaged neighborhoods but also what is present in advantaged neighborhoods.

To date, the neighborhood effects literature has focused on the Truly Disadvantaged. The present research suggests that no matter how many individual families are moved into "better" neighborhoods or how many individual neighborhoods are targeted for economic development, neighborhoods will perpetuate generational poverty and generational wealth as long as neighborhood inequality exists. Neighborhood inequality itself is the problem. If, like Wilson, we want to rectify the injustices faced by the Truly Disadvantaged, we must also consider the Truly Advantaged.

Chapter 3

The Tipping Point: Examining and Evaluating Demographic Measurements of Poor Neighborhoods

3.1. Executive Summary

Sociologists and demographers concerned about inequality have long argued that the concentration of poverty is an important mechanism perpetuating the intergenerational transmission of socioeconomic status. The majority of these scholars have operationalized impoverished neighborhoods as places where at least 40 percent of the population lives beneath the federal poverty line. However, to date, scholars have not conducted a thorough empirical examination of various tipping points. The present study uses the geocoded Panel Study of Income Dynamics to conduct an empirical examination of childhood neighborhood effects on educational attainment. Results indicate that the 5

percent threshold best captures the categorical distinction between “poor” and “non-poor” neighborhoods. This study then utilizes the U.S. Census to examine whether this alternative definition of impoverished neighborhoods changes traditional findings regarding metropolitan level trends in concentrated poverty. Findings suggest that this alternative measurement provides substantive differences in correlations between metropolitan factors and exposure to poverty. The paper concludes with a discussion of the implications of these findings on scholarly research and policy.

3.2. Introduction

Over the last three decades, the literature has repeatedly demonstrated that the concentration of poor residents into high-poverty neighborhoods has detrimental effects on socioeconomic status and physical wellbeing (Wilson 1987; Brooks-Gunn et al. 1993; Turley 2003; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Kimbro, Denney and Panchang 2012; Sampson 2012; Massey et al 2013; Chetty, Hendren and Katz 2016). Thus, demographic research has tracked trends in poverty concentration across time and regions to identify the factors perpetuating concentrated poverty and in turn to identify possible avenues for curtailing its perpetuation (Kasarda 1993; Massey and Denton 1993; Holloway et al. 1998; Quillian 1999; Jargowsky 2003; Reardon and Bischoff 2011; Dwyer 2012; Lichter, Parisi and Taquino 2012; Firebaugh and Farrell 2016; Iceland and Hernandez 2016). Yet, quantitative assessments have not sufficiently evaluated whether their operationalizations of concentrated poverty reflect the most pronounced schism between impoverished and non-poor neighborhoods. Instead, the

majority of studies conceptualize poor neighborhoods as categorically distinct from non-poor neighborhoods and operationalize them as neighborhoods with at least 40 percent of the residents living at or below the federal poverty line (Danziger and Gottschalk 1987; Quillian 1999; Jargowsky 2003; Iceland and Hernandez 2016).

This approach traces back to the 1980s when suburbanization and deindustrialization drew attention to the disinvestment in U.S. inner cities (Danziger and Gottschalk 1987; Wilson 1987). Initial studies utilizing 40 percent as the cutoff point to identify problematic neighborhoods did not provide an empirical rationale for this particular tipping point. However, in 1991, Jargowsky and Bane provided a justification by touring cities and reporting that, based on visual inspection of local housing stocks, neighborhoods with poverty rates of 40 percent and higher looked considerably more dilapidated than neighborhoods with less poverty (Iceland and Hernandez 2016). This threshold then became the standard for quantitative, comparative research, thwarting subsequent consideration of whether this tipping point of 40 percent best distinguishes detrimental from non-detrimental neighborhoods when it comes to residents' socioeconomic and physical wellbeing.

Utilizing the restricted geocoded Panel Study of Income Dynamics (PSID), the U.S. Decennial Census, and the American Community Survey, I empirically explore which neighborhood poverty threshold, or tipping point, best captures the most variation in the relationships between childhood neighborhoods and residents' adult educational attainments. Results indicate that the 5 percent poverty rate is the tipping point that captures the most variation in adult educational attainment. This alternative threshold is radically different from the traditional tipping point of 40 percent. Nevertheless, this

alternative tipping point would make little substantive difference on demographic studies of metropolitan level poverty concentration if the metropolitan level trends using this alternative cutoff point are parallel to the metropolitan trends using the traditional threshold. Thus, I take this study one step further and also explore metropolitan level demographic trends in poverty concentration using both the traditional and alternative tipping points. Findings show significant differences between the two thresholds, suggesting that the thresholds used by researchers influence their theoretical conclusions.

The point of this thorough quantitative analysis of neighborhood poverty measures is twofold. First, this study provides a refined set of statistical tools with which to evaluate neighborhood effects on social outcomes and to examine metropolitan levels of concentrated poverty. Second, this research directs conceptual and theoretical work on neighborhood effects and neighborhood inequality to consider an additional set of neighborhood and metropolitan level mechanisms and dynamics that have yet to be discussed or studied in the literature. I conclude by discussing these mechanisms and dynamics as well as their implications on theory.

3.3. Poverty Concentration: Why It Matters and How It's Measured

Starting with Wilson's (1987) assertions that neighborhood conditions influence residents' outcomes above and beyond their individual or familial characteristics, a plethora of studies have empirically substantiated that neighborhood economic status influences residents' socioeconomic and physical wellbeing (Brooks-Gunn et al. 1993; LeClere, Rogers and Peters 1997; Mahatmya and Lohman 2012; Massey et al 2013;

Martens et al 2014; Chetty, Hendren and Katz 2016). In particular, one's childhood neighborhood influences educational attainment—a common variable in neighborhood effects studies because of its direct relationship with neighborhood context and its influence on other socioeconomic and health outcomes (Ainsworth 2002; Turley 2003; Stewart, Stewart and Simons 2007; Ainsworth 2010; Casciano and Massey 2012).

Scholars argue that both the formal resources and socialization in childhood neighborhoods shape educational attainment. Specifically, resources in neighborhood schools such as qualified teachers, textbooks and technology shape children's educational experiences and achievement. Yet, even if students do not attend their local public schools, the social norms within their communities affect the time students spend reading and studying as well as their educational aspirations. These norms, in turn, influence students' educational success (Ainsworth 2002; Ainsworth 2010; Casciano and Massey 2012; Brattbakk and Wessel 2013). Additionally, neighborhood-based social networks and educational expectations determine the information and support students receive regarding higher education (Ainsworth 2002; Andersson and Subramanian 2006; Brattbakk and Wessel 2013). In short, scholars argue that impoverished neighborhoods lack the resources, social norms, networks and expectations that enable children to complete high levels of educational attainment.

Given the negative effect of impoverished neighborhoods on socioeconomic status, demographers have paid close attention to their rise and distribution. That is, they explore trends in the number of concentrated poor census tracts over time and across geographic regions (Kasarda 1993; Massey and Denton 1993; Holloway et al. 1998; Quillian 1999; Jargowsky 2003; Reardon and Bischoff 2011; Dwyer 2012; Lichter, Parisi

and Taquino 2012; Firebaugh and Farrell 2016; Iceland and Hernandez 2016).

Additionally, scholars will use longitudinal and cross-sectional analyses to illuminate how various county and metropolitan features such as income and racial segregation levels, economic performance, racial inequality and racial proportions contribute to the concentration of poverty (Firebaugh and Farrell 2016; Iceland and Hernandez 2016).

These descriptive reports and analytical studies empirically operationalize high poverty neighborhoods as quantifiably and categorically distinct from all other communities. Although some variation in measurement does exist (e.g. Massey 1996; St. John 2002; Dwyer 2010; Reardon and Bischoff 2011; Dwyer 2012; Lichter, Parisi and Taquino 2012), the vast majority of this scholarship identifies impoverished neighborhoods as those where the poverty rate exceeds a particular tipping point. The most common definition of impoverished communities is those with at least 40 percent of the residents living at or below the federal poverty line (Danziger and Gottschalk 1987; Quillian 1999; Jargowsky 2003; Iceland and Hernandez 2016). At times, scholars also use a 20, 30 or 35 percent poverty rate as their tipping point. Additionally, some researchers operationalize neighborhood poverty as an ordinal variable. For example, some scholars define non-poor neighborhoods as those with a poverty rate under 20 percent, moderately poor neighborhoods as those with a poverty rate between 20 and 39 percent and extremely poor neighborhoods as those with at least a 40 percent poverty rate (Kasarda 1993; Krivo and Peterson 1996; Massey 1996). Yet, no matter the specific thresholds these scholars explicitly and implicitly argue, poor and extremely poor neighborhoods are categorically distinct from all other neighborhoods. Furthermore,

these scholars argue that these poor neighborhoods have negative implications on their residents' wellbeing.

Nevertheless, research has not empirically linked these chosen thresholds to the theoretically important relationship between neighborhood characteristics and residents' outcomes. As mentioned above, some work justifies the 40 percent tipping point by citing Jargowsky and Bane's (1991) tour of poor neighborhoods, which demonstrated a categorical distinction between neighborhoods above and below 40 percent poor, based on their dilapidated housing stock. Additionally, other works using cross-sectional surveys have operationalized neighborhood poverty as an ordinal variable and examined whether their most impoverished neighborhood category had distinct effects on residents' job networks (Tigges, Browne and Green 1998; Elliott and Sims 2001).²⁴ The present study builds upon these previous investigations to provide a more thorough evaluation of neighborhood poverty in a particular realm of interest, educational attainment.

In particular, this research advances the investigation of impoverished neighborhood tipping points with two key methodological refinements. First, this study uses longitudinal data. Previous research has demonstrated neighborhood effects are cumulative across time and generations (Entwisle, Alexander, and Olson 2005; Sharkey

²⁴ In general, results of these studies indicate significant differences between the least poor and most impoverished neighborhoods but not between the median-level poverty neighborhoods and the high-poverty communities. The one exception being when examining the inclusion of college educated individuals in respondents' networks. For this one outcome, the most impoverished neighborhoods are distinguishable from neighborhoods with low- and median-poverty levels. In short, the majority of these results point to the distinction of low-poverty communities from all other neighborhoods, not a categorical distinction of high-poverty neighborhoods from all other communities.

and Elwert 2011; Wodtke, Harding and Elwert 2011; Sampson 2012; Sharkey 2013).

Thus, I utilize longitudinal data to examine how childhood neighborhood poverty (from ages zero to 18) influences adult educational attainment. Second, this research employs non-nested model selection techniques to evaluate all possible tipping points (Vuong 1989; Clarke 2003; Wooldridge 2010; Howell and Emerson 2017). Unlike previous examinations of neighborhood tipping points that only examined a small set of thresholds, this exploratory approach allows this study to examine all possibilities. In doing so, this research moves beyond mere technical innovations and illuminates potential theoretical inquiries.

3.4. Data and Methods

As noted above, the aim of this research is twofold: to refine statistical measures of neighborhood poverty and to direct theoretical work to consider neighborhood and metropolitan mechanisms underexplored in the literature. To achieve these aims, I begin with two distinct empirical analyses. The first analysis explores neighborhood tipping points and the second employs these distinct tipping points in an examination of trends in metropolitan level concentrated poverty. In what follows, I will discuss the data and methods of the first analysis followed by the second.

3.4.1. Analysis One: Impoverished Neighborhood Tipping Points

The purpose of this first analysis is to identify the tipping point at which impoverished communities become quantifiably and categorically distinct from all other neighborhoods. To do this, I employ non-nested model testing. Non-nested model testing has been used in international relations (Vuong 1989; Clarke 2003), economics (Wooldridge 2010) and sociology (Howell and Emerson 2017) to distinguish between different operationalizations of a key variable. When previous scholarship has proposed various operationalizations of a single variable each with valid theoretical reasoning, this method enables scholars to empirically distinguish the utility of these operationalizations in empirical research. Conceptually speaking, the method runs a series of models. Each model uses one of the proposed operationalizations of the variable in question. Models are then evaluated based on the amount of variation in the dependent variable they explained relative to their degrees of freedom.

I employ this method in this examination of neighborhood tipping points by comparing models with various definitions of neighborhood poverty each predicting educational attainment. To predict educational attainment, I utilize the most common method in the neighborhood effects research—geocoded, longitudinal data (e.g. Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011). Specifically, I use the Panel Study of Income Dynamics (PSID)—the longest running national representative geocoded data set. Beginning in 1968, the PSID has followed the same families and gathered information on their socioeconomic status. As children grew up and established their own households, PSID also surveyed these families resulting in some individuals having annual data points from their birth through their early 40s.

The present study is interested in investigating how childhood neighborhoods influence adult educational attainment. To conduct this investigation, respondents need to have been included in the survey for their entire childhoods as well as their early adulthoods. Additionally, to reduce extraneous factors of time frame, I select only respondents born between 1975 and 1985 who were still in the sample at age 26, which leaves 2,367 individuals. Reflecting the racial dynamics of the United States in 1968, over 90 percent of the respondents in the initial PSID sample identified as either White or Black.²⁵ In the initial sample, too few respondents identify as each of the other racial groups to analyze these groups separately. Hence, I follow the precedent in the literature of excluding these 140 individuals and only using respondents who identify as White or Black (Sharkey 2013).

3.4.1.1. Neighborhood Poverty Measures

Neighborhood poverty can have contemporary and long term influences on residents' wellbeing. Yet, unlike adults who have some, although at times limited, agency in their residential choices, children must live with their guardians. Hence, the effects impoverished childhood neighborhoods have on residents' long term wellbeing is particularly problematic. Thus, the present study focuses on how growing up in impoverished neighborhoods influences adult educational attainment. Building off the

²⁵ In 1997 and 1999, the PSID added immigrant families to their sample to adjust for U.S. demographic changes. Nevertheless, these recent additions do not have data on their residential locations from the 1980s and 1990s. Thus, I cannot include them in my study.

finding that childhood neighborhood effects are cumulative across one's childhood (Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011), this study conceptualizes childhood neighborhoods as a composite of all neighborhoods that respondents lived in from birth until they moved out of their parents' home or turned 18.

Neighborhoods are operationalized as U.S. census tracts, and data on these tracts come from 1980, 1990 and 2000 Census Long Form as well as the 2005-2010 American Community Survey (ACS). All census tract boundaries are normalized to the 2010 census tracts using Logan, Xu and Stults' (2014) cross-walk files. I utilized linear imputation to estimate the neighborhood poverty for all inter-census years. In linear imputation calculations, the 2005-2010 ACS values were assigned to the year 2007.

For every year of the respondents' childhood, I linked the respondent to the census tract of their primary residence. Thus, for every year of their childhood, I know what proportion of their residential census tract lived below the federal poverty line. This means respondents' neighborhood poverty can vary annually because of demographic changes in their same neighborhood *or* because of residential moves between neighborhoods. I then created a composite childhood neighborhood poverty rate by calculating the mean of all the annual neighborhood poverty rates across the respondents' childhood. In other words, I use one number—which is continuous—to represent the respondents' childhood neighborhood poverty rate.

As seen in Table 3.1, on average, residents' childhood neighborhood poverty rate was 16 percent, which approximates the national poverty rates over this time period. Yet some respondents grew up in neighborhoods where approximately 1 percent of the population lived at or under the poverty line while others grew up in neighborhoods

where 77 percent of the population lived in poverty. This vast range in neighborhood poverty is why concentrated poverty has become of central interest in social research and policy.

	Mean (Stan. Dev.)	Min	Max
Dependent Variables			
Completed Years in School at Age 26	13.56 (2.10)	5	17
Childhood Neighborhood Poverty			
Poverty Rate	0.16 (0.11)	0.01	0.77
Individual Demographics			
Black	0.41 (0.49)	0	1
Female	0.52 (0.50)	0	1
Childhood Parental Controls			
Parents' Income	61,483 (51,757)	0	626,946
Parents' Years in School	13.19 (2.32)	0	17
Proportion of Years Parents Married	0.74 (0.35)	0	1
Number of Siblings	2.37 (0.90)	0	6.73
Number of Moves	2.84 (2.68)	0	15
N—Individuals (Families)	2238 (972)		

Table 3.1 – Descriptive Statistics of PSID Respondents.

To test impoverished neighborhood tipping points, I use each respondents' composite childhood neighborhood poverty rate to create a series of dichotomous and ordinal variables. I began by operationalizing impoverished neighborhoods as a dichotomous variable. Specifically, I explored 70 different dichotomous operationalizations of poor neighborhoods. The first operationalization uses a 1 percent tipping point and defines poor neighborhoods as those with at least 1 percent of the population living at or under the poverty line. The second operationalization uses a 2 percent threshold defining poor neighborhoods as those with at least 2 percent poverty. I continued to increase the "tipping point" by one percent until I reached 70 percent. That

is, the final dichotomous operationalization of neighborhood poverty defines poor neighborhoods as those with at least 70 percent of the population living at or below the federal poverty line.

I then explored ordinal operationalizations of neighborhood poverty. First, I examined ordinal variables with three categories. Using a similar method as above, I estimated models using every possible three-category classification scheme between 1 and 70 percent. For example, the first classification denotes non-poor neighborhoods as those with a poverty rate less than 1 percent, moderately poor neighborhoods as with a poverty rate above 1 percent but below 2 percent and extremely poor neighborhoods as those with a poverty rate of at least 2 percent. This pattern continues until the final classification of non-poor neighborhoods as those with a poverty rate less than 69 percent, moderately poor neighborhoods as those with a poverty rate between 69 and 70 percent and extremely poor neighborhoods as those with a poverty rate at or above 70 percent. The interim includes more conventional three-category operationalizations of neighborhood poverty such as less than 20 percent poor, between 20 and 40 percent poor and at least 40 percent poor. Using this method, I examine a total of 2,415 three-category ordinal operationalizations of neighborhood poverty.²⁶ I repeat this same methodology for ordinal variables with four categories which produces 54,740 different operationalizations of neighborhood poverty.²⁷

²⁶ ${}_nC_r = \frac{n!}{r!(n-r)!} = \frac{70!}{2!(70-2)!} = 2,415$

²⁷ ${}_nC_r = \frac{n!}{r!(n-r)!} = \frac{70!}{3!(70-3)!} = 54,740$

3.4.1.2. Educational Attainment

Although neighborhoods influence a wide variety of residents' socioeconomic and health outcomes, educational attainment is a common outcome of interest because of its influence on other forms of socioeconomic status and physical wellbeing. Educational attainment is operationalized as completed years of school at age 26. The variable ranges from five to 17, with all respondents with more than a bachelor's degree assigned the value of 17 years (see Figure 3.1). The PSID uses this upper censorship for privacy reasons, but it has the advantageous side effect of ensuring the right skew in the dependent variable does not bias results. As expected, the modal year of completed years in school is 12 years or high school diploma (34 percent of the total sample) followed by 16 years or bachelor's degree (20 percent of the total sample).

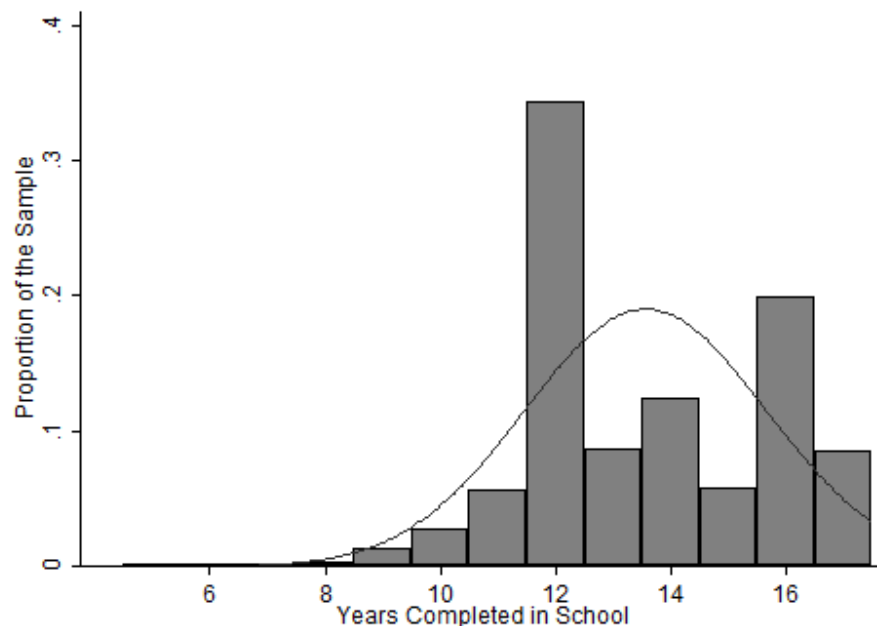


Figure 3.1 – Distribution of Educational Attainment Compared to The Normal Curve.

To ensure the distribution of educational attainment is not influencing the results, I run a series of sensitivity tests operationalizing educational attainment as categorical. In addition to their empirical utility, these additional models enable me to examine whether the results differ when education is conceptualized as completed degree instead of completed years in school. Specifically, I run all analyses with education defined as dichotomous (i.e. less than high school versus high school diploma and less than college versus college) and ordinal (less than high school, high school diploma, some college, bachelor's degree, and graduate school). Results from these logistic and ordered logistic regressions were comparable to those presented here and are available upon request. Furthermore, as seen in Figure 3.2, the bivariate distribution of childhood neighborhood poverty and educational attainment does not suggest a bimodal distribution that would preclude the use of ordinary least squares regressions. Hence, for ease of reader comprehension educational attainment is operationalized as continuous.

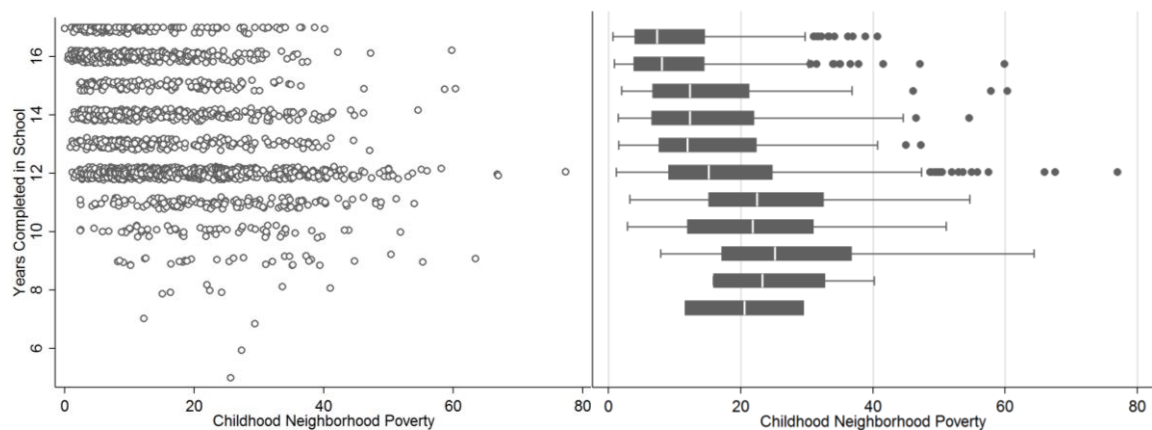


Figure 3.2 – Bivariate Distribution of Educational Attainment Across Childhood Neighborhood Poverty.

3.4.1.3. *Controls*

To isolate the influences of neighborhoods from familial and individual characteristics, I control for various family and individual factors. On the individual level, I control for race and gender. Although both race and gender are multifaceted, fluid and complex identities, for the purposes of this study, these variables are conceptualized as mutually exclusive and binary. In other words, race is measured as White or Black and gender is operationalized as male or female. This approach has shortcomings but more complex operationalizations are not possible given the constraints of the data.

In addition to these individual characteristics, I control for five family level factors. Like the composite neighborhood poverty variable, childhood family characteristics are averages across respondents' entire childhoods (ages zero to 18). Using composite scores allows the research to capture variation across time. Specifically, these controls include parental income, education, marital status, number of siblings, and number of moves. The composite parental income was derived by summing the mother's and father's income in each year. All values were normalized to 2012 dollars to adjust for inflation. Then I used all the annual parental incomes to calculate the mean parental income across the respondent's childhood. Likewise, for each year, I compare the father's and mother's years of education completed and took the highest attainment as the familial educational attainment. These annual educational attainments were then averaged across the years. Introducing a temporal approach into the parental educational control variable allows for differences between parents who completed their college education before

having kids and those who went back to school when their children were older.²⁸ In comparable fashion, parental marital status is measured as the proportion of years the parents were married during the respondents' childhood. Number of siblings is the average number of children living in the household across respondents' childhood. Finally, the number of moves is measured as the number of times the respondent moved during their childhood. For all parental controls, the descriptive Table 3.1 includes raw scores although they are standardized in all models.

Controlling for parental characteristics enables us to differentiate familial influences from neighborhood effects. Nevertheless, it also provides a conservative estimate of neighborhood effects. That is, parents' childhood neighborhoods shape where they raise their children (Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011). Data on the childhood neighborhoods of the parents in my sample is not available so I am unable to take these locations into consideration. Because of this, my estimates are likely conservative.

²⁸ Including parental education as a control arguably results in change models predicting intergenerational educational mobility because they examine educational attainment relative to parental education. Yet, strictly speaking a mobility model would require operationalizing parental and child education identically. For these models, that would require operationalizing parental education as the completed years in school when the parents were 26 years old. Yet, I choose to use this temporal measurement to capture how parental education attained later in life might also be influencing their children's educational attainment. For this reason, I do not conceptualize these models as estimating educational mobility, although they are similar to educational mobility models.

3.4.1.4. Statistical Modeling

Since educational attainment is a continuous variable, all models are ordinary least squares regressions. Nevertheless, given that some of the respondents are siblings, I used multilevel modeling to account for multiple respondents within one family. Specifically, I used the Stata command `xtreg` to estimate these models. All models are identical in their dependent and control variables. Yet, how impoverished neighborhoods are operationalized differs across the models. As outlined above, I have a total of 57,225 operationalizations of neighborhood poverty. I compared these various tipping points using Wooldridge's (2010) equations applying Vuong's (1989) likelihood ratio test to multilevel models. I also used the models' Wald test score to illuminate which tipping points with the same degrees of freedom explain the most variation in educational attainment.

3.4.2. Analysis Two: Metropolitan Trends in Poverty Concentration and Exposure

After identifying the tipping point that distinguishes impoverished neighborhoods from other communities, I turn to my second analysis which explores whether the proposed alternative definition of neighborhood poverty changes trends in metropolitan poverty concentration. As mentioned above, since the 1980s, demographic reports and studies have explored trends in poverty concentration across metropolitan areas. Most often these studies define metropolitan concentrated poverty as the proportion of the metropolitan area living in neighborhoods that are at least 40 percent poor (Danziger and Gottschalk 1987; Quillian 1999; Jargowsky 2003; Iceland and Hernandez 2016). My first analysis

points towards a radically different tipping point than the traditional threshold of 40 percent. In fact, it suggests the categorical distinction between neighborhoods occurs at the 5 percent tipping point. In other words, the exclusion of poverty from certain neighborhoods drives the correlation between neighborhood poverty and educational attainment. Hence, this second analysis asks whether the conclusions of metropolitan level studies are altered when poverty exposure is considered instead of poverty concentration.

It is possible that the proportion of the metropolitan area living in tracts that are at least 40 percent poor strongly correlates with the metropolitan proportion living in tracts that are at least 5 percent poor. If this is the case, the metropolitan level studies of poverty concentration will be comparable to studies examining poverty exposure. However, if this is not the case, using the alternative neighborhood tipping point might lead to additional unexplored mechanisms perpetuating inequality in U.S. metropolitan areas.

To adjudicate these possibilities, I compare metropolitan trends in poverty concentration. The data for this analysis comes from the 1980-2000 U.S. decennial census long form (summary file 3) and the 2005-2010 American Community Survey (ACS) summary files. As with my first analysis, neighborhoods are defined as census tracts and all census tract and metropolitan area boundaries are normalized to the 2010 census tracts using Logan, Xu and Stults' (2014) crosswalk files. However, for this second analysis, the unit of analysis is metropolitan areas. I used cross-sectional ordinary least squares regressions and longitudinal fixed-effects regressions to investigate how metropolitan characteristics influence metropolitan poverty concentration and exposure.

3.4.2.1. Concentrated Poverty and Exposed Poverty

I defined metropolitan level poverty concentration as the proportion of the metropolitan residents living in impoverished neighborhoods. Using the traditional threshold of 40 percent, I calculated the proportion of metropolitan residents that live in neighborhoods where at least 40 percent of the neighborhood residents live in poverty. Likewise, I define metropolitan level poverty exposure as the proportion of the metropolitan residents living in neighborhoods with moderate or extreme poverty rates. Using the alternative tipping point, I calculated the proportion of metropolitan residents living in neighborhoods with a poverty rate of 5 percent or greater.

3.4.2.2. Metropolitan Characteristics

Mirroring previous demographic examinations of metropolitan level, concentrated poverty (e.g. Lichter, Parisi and Taquino 2012; Iceland and Hernandez 2016), I examined how ecological metropolitan characteristics influence the proportion of the metropolitan population living in poor neighborhoods and the proportion of the metropolitan population living in neighborhoods with little to no poverty. Following previous scholarship, I included the overall metropolitan poverty rate and racial proportions. Specifically, I included the proportion of the metropolitan area that identifies as non-Hispanic White, the metropolitan proportion that identifies as non-Hispanic Black, the proportion of the metro area that identifies as Hispanic and the proportion that identifies as all other racial groups including all multiracial individuals. In the models, the

proportion of the metropolitan area that identifies as all other racial groups is excluded and thus serves as a reference group.

	1980	1990	2000	2010
Poverty Rate	0.12 (0.04)	0.13 (0.05)	0.13 (0.04)	0.15 (0.04)
Racial Groups				
<i>White</i>	0.83 (0.15)	0.81 (0.16)	0.76 (0.17)	0.71 (0.18)
<i>Black</i>	0.09 (0.10)	0.10 (0.10)	0.11 (0.11)	0.11 (0.11)
<i>Latino</i>	0.06 (0.12)	0.07 (0.13)	0.10 (0.15)	0.13 (0.16)
<i>Other</i>	0.02 (0.05)	0.03 (0.05)	0.04 (0.06)	0.04 (0.06)
Proportion in Central City	0.42 (0.21)	0.39 (0.19)	0.37 (0.19)	0.35 (0.19)
Owner Occupancy Rate	0.67 (0.06)	0.66 (0.06)	0.54 (0.06)	0.68 (0.06)
Nativity Proportion	0.95 (0.04)	0.94 (0.06)	0.92 (0.07)	0.90 (0.08)
Proportion with Bachelor's Degree	0.12 (0.04)	0.15 (0.05)	0.19 (0.06)	0.21 (0.06)
Median Income	45306 (6855)	49225 (9832)	54615 (9843)	51980 (9468)
Population Size (000s)	519 (1281)	600 (1394)	684 (1552)	758 (1657)
Region				
<i>Northeast</i>	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)	0.10 (0.30)
<i>Midwest</i>	0.22 (0.42)	0.22 (0.42)	0.22 (0.42)	0.22 (0.42)
<i>South</i>	0.40 (0.49)	0.40 (0.49)	0.40 (0.49)	0.40 (0.49)
<i>West</i>	0.24 (0.43)	0.24 (0.43)	0.24 (0.43)	0.24 (0.43)
Number of Metropolitan Areas	339			

Table 3.2 – Descriptive Statistics of Metropolitan Areas.

Additionally, I included the proportion of the metropolitan area that lives within the center city (defined as the municipality with the largest population), the metropolitan homeowners occupancy rate, metropolitan nativity proportion, and the proportion of metropolitan residents with a bachelor's degree. Moreover, I controlled for the median household income in the metropolitan area and the total metropolitan population size. Both median household income and total population are right skewed. Using the ladder command in Stata which searches a subset of ladder powers, I determined that the transformation that best fits the distribution of both these variables was a negative square root. Hence, in the models, the transformed versions of these variables were utilized.

Finally, I included the geographic region of the metropolitan area defining region as Northeast, Midwest, South and West.²⁹ For a summary of these metropolitan descriptive statistics and their changes over time see Table 3.2. In the models, all continuous variables—including the dependent variable—are standardized to enable comparisons.

3.5. Results

3.5.1. Analysis One: Impoverished Neighborhood Tipping Points

To begin my exploration of neighborhood tipping points, I use my PSID sample and estimate a series of models predicting respondents' educational attainment. I start with the dichotomous definitions of neighborhood poverty. As outlined in the above Data and Methods section, I used 70 different dichotomous operationalizations of neighborhood poverty. Each model has the same control variables but varies in its definition of childhood neighborhood poverty. I then compared the amount of variation in educational attainment that each model explained using the Wald test. As visualized in Figure 3.3, the tipping point of 5 percent—that is defining non-poor neighborhoods as those with less than 5 percent poverty and poor neighborhoods as those with at least a 5 percent poverty rate—explains the most variation in educational attainment. Starting at the 1 percent threshold, the variation explained by the models increases as the tipping point approaches 5 percent. At this point, the variation explained by the models begins to decrease until the

²⁹ For metropolitan areas that are located in multiple regions (i.e. Cincinnati, Ohio and Louisville, Kentucky), the region of the most populated city was utilized as the metropolitan region.

tipping point is approximately 15 percent, after which the explained variation levels out. In other words, all the thresholds above 15 percent poverty explain equitable amounts of the variation in educational attainment.

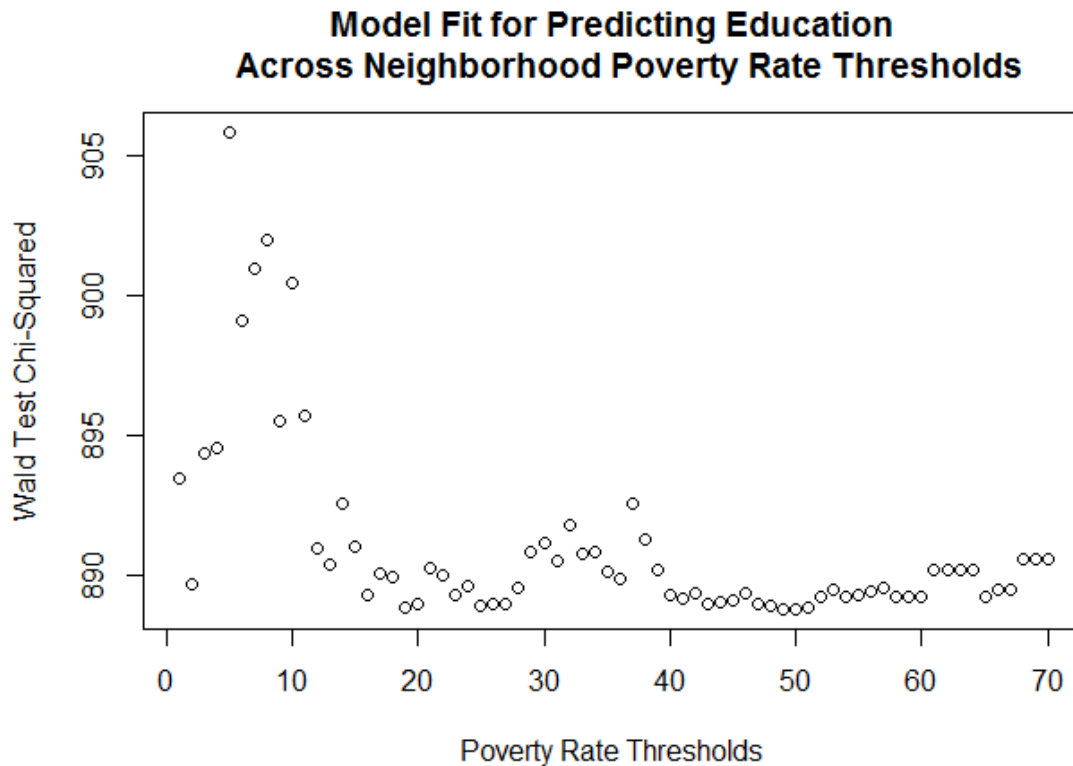


Figure 3.3 – Wald Test Results Comparing Models Predicting Educational Attainment by Neighborhood Poverty.

To help illustrate these findings, consider four of the 70 models run. First, I examine the model that accounts for the most variation in educational attainment—the 5 percent threshold. In this model, poor neighborhoods are those with at least 5 percent of the population living in poverty. In this sample, 355 respondents grew up in neighborhoods with a poverty rate less than 5 percent while 1,883 respondents grew up in

neighborhoods which had a poverty rate of at least 5 percent. As expected, the 1,883 respondents growing up in neighborhoods with at least a 5 percent poverty rate completed less education than their counterparts in neighborhoods with less than 5 percent poverty. As seen in the first model of Table 3.3, even when individual and family characteristics are held constant, the relationship between childhood impoverished neighborhoods and educational attainment is statistically significant ($p\text{-value} = 0.004$). Additionally, the relationships between all the control variables and educational attainment are in the expected directions; building confidence that the model reflects the variation in educational attainment observed in other studies.

Next, I consider the second model in Table 3.3 which uses a 10 percent threshold for defining poor neighborhoods. That is, in this model impoverished communities are defined as those where at least 10 percent of the population lives at or under the federal poverty line. Using this definition, I once again find that respondents ($n = 1,346$) who grew up in poor neighborhoods complete less education than their counterparts in non-poor communities. Furthermore, the relationship between all the control variables and educational attainment is nearly identical to the first model. In fact, the coefficients for the controls are comparable across all the models. However, although still statistically significant ($p\text{-value} = 0.011$), the magnitude of the relationship between poor neighborhoods and educational attainment is smaller than the previous model that utilized 5 percent poverty as the tipping point for poor neighborhoods. Likewise, as demonstrated by the model fit indicators (i.e. the Wald test and R^2), this model explains less of the overall variation in educational attainment than the previous model.

	Tipping Points			
	5 Percent	10 Percent	20 Percent	40 Percent
Poor Neighborhood	-0.35 (0.12)*	-0.25 (0.10)*	-0.01 (0.11)	-0.05 (0.21)
Individual Demographics				
Black	-0.04 (0.10)	0.02 (0.11)	-0.07 (0.11)	-0.08 (0.10)
Female	0.58 (0.07)*	0.58 (0.07)*	0.58 (0.07)*	0.58 (0.07)*
Childhood Controls				
Parents' Income	0.25 (0.05)*	0.26 (0.05)*	0.29 (0.05)*	0.29 (0.05)*
Parents' Years in School	0.71 (0.05)*	0.71 (0.05)*	0.73 (0.05)*	0.73 (0.05)*
Parents' Years Married	0.21 (0.05)*	0.20 (0.05)*	0.19 (0.05)*	0.19 (0.05)*
Number of Siblings	-0.15 (0.04)*	-0.14 (0.04)*	-0.14 (0.04)*	-0.14 (0.04)*
Number of Moves	-0.16 (0.04)*	-0.16 (0.04)*	-0.16 (0.04)*	-0.16 (0.04)*
Constant	13.57 (0.12)	13.40 (0.08)	13.30 (0.07)	13.30 (0.07)
Within R ²	0.0840	0.0842	0.0838	0.0838
Between R ²	0.3937	0.3917	0.3894	0.3895
Wald Chi ²	905.84	900.45	888.98	889.28
N—Individuals(Families)	2238 (972)	2238 (972)	2238 (972)	2238 (972)

*Denotes the coefficient is statistically significantly different from zero with a P-Value ≤ 0.05 .

Table 3.3 – Coefficients from Regressions Predicting Educational Attainment Using Dichotomous Poverty Measures of Childhood Neighborhoods.

In like manner, the next two models of Table 3.3—those employing the 20 and 40 percent tipping points—explain even less of the variation in educational attainment. Specifically, the third model which defines poor neighborhoods as those with at least a 20 percent poverty rate, finds a null relationship between poor neighborhoods and educational attainment. Likewise, the final model of Table 3.3, which operationalizes poor neighborhoods as those with at least 40 percent of the population living at or below the federal poverty line, finds no statistically significant correlation between childhood impoverished neighborhoods and educational attainment. Additionally, both these models explain less of the variation in educational attainment than the model which uses 5 percent poverty as its tipping point. This suggests that neighborhoods above and below

the 40 percent poverty marker are not quantifiably and qualitatively different when it comes to their influence on children's educational attainment and for that matter neither are neighborhoods above and below the 20 percent poverty threshold.

It must be noted that only 84 respondents (4 percent of the sample) grew up in neighborhoods where at least 40 percent of the population lived at or below the federal poverty line. This small count increases the standard errors of the impoverished neighborhood coefficient which in turn makes its statistical significance less likely. Nevertheless, the coefficient itself is seven times smaller than its corresponding coefficient in Model 1 (the model using the 5 percent tipping point). Additionally, the model operationalizing poor neighborhoods as at least 20 percent poor also has null results despite the 687 respondents (31 percent of the sample) that live in neighborhoods classified as poor. This is a more equitable distribution of respondents across neighborhood types than the model using the 5 percent tipping point. Hence, these unconventional results cannot be accounted for by inadequate sample sizes.

	Tipping Points		
	(< 5; 5-10; 10 +)	(< 10; 10-40; 40 +)	(< 20; 20-40; 40 +)
Neighborhood Poverty			
Moderate Poverty	-0.17 (0.10)	0.04 (0.21)	0.05 (0.21)
Extreme Poverty	-0.44 (0.13)*	0.29 (0.23)	0.06 (0.22)
Individual Demographics			
Black	0.03 (0.11)	0.03 (0.11)	-0.07 (0.11)
Female	0.58 (0.07)*	0.58 (0.08)*	0.58 (0.07)*
Childhood Controls			
Parents' Income	0.24 (0.05)*	0.26 (0.05)*	0.29 (0.05)*
Parents' Years in School	0.70 (0.05)*	0.71 (0.05)*	0.73 (0.05)*
Parents' Years Married	0.21 (0.05)*	0.20 (0.05)*	0.19 (0.05)*
Number of Siblings	-0.15 (0.04)*	-0.14 (0.04)*	-0.14 (0.04)*
Number of Moves	-0.16 (0.04)*	-0.16 (0.04)*	-0.16 (0.04)*
Constant	13.14 (0.09)	13.11 (0.22)	13.23 (0.22)
Within R ²	0.0843	0.0841	0.0837
Between R ²	0.3942	0.3918	0.3895
Wald Chi ²	909.34	900.08	888.69
N—Individuals(Families)	2238 (972)	2238 (972)	2238 (972)

*Denotes the coefficient is statistically significantly different from zero with a P-Value ≤ 0.05 .

Table 3.4 – Coefficients from Regressions Predicting Educational Attainment Using Ordinal Poverty Measures of Childhood Neighborhoods.

Nevertheless, this does not preclude the possibility that the categorical distinctions of neighborhood poverty are ordinal. In fact, it might be the case that a three- or four-category classification of neighborhood poverty is required to highlight the distinct influence that high concentrations of poverty has on residents. To test this possibility, I ran models using all 2,415 possible three-category operationalizations of neighborhood poverty. Of all these models, the model that explains the most variation in educational attainment defines non-poor neighborhoods as less than 5 percent poor, moderately poor neighborhoods as at least 5 percent poor but less than 10 percent poor and extremely poor neighborhoods as at least 10 percent poor. Specifically, as seen in the

first model of Table 3.4, residents who grow up in extremely poor neighborhoods (communities with at least 10 percent poverty) complete statistically significantly less education than their counterparts in non-poor neighborhoods ($p\text{-value} = 0.001$) and moderately poor neighborhoods ($p\text{-value} = 0.033$).

Furthermore, once again I find using the more conventional categories for neighborhood poverty explains less of the variation in educational attainment. For example, when non-poor neighborhoods are defined as those with less than 10 percent poverty, moderate poverty as neighborhoods with 10 to 40 percent poverty and extreme poverty as at least 40 percent poverty, the coefficients for moderate and extreme poverty are not significant. Additionally, as signified by the model fit indicators, seen in Table 3.4, this model explains less of the overall variation in educational attainment. These patterns are also true for other traditional ordinal definitions such as less than 20 percent poor; 20-40 percent poor and 40 percent or more poor. These results support the initial analysis and reaffirm the finding that the tipping point between non-poor and poor neighborhoods is at a much lower level of poverty than previously thought. However, to exhaust all possible options, I also examined the ordinal variables with four categories.

Comparing the 54,740 models that use all possible four-category ordinal variables, the model that explained the most variation in educational attainment defined non-poor neighborhoods as those with less than 5 percent poverty, low poverty neighborhoods as those with at least 5 and less than 10 percent poverty, moderate poverty neighborhoods as those with at least 10 and less than 12 percent poverty and high poverty neighborhoods as those with at least 12 percent poverty. These results are comparable to

the previous findings, once again suggesting that the tipping points for neighborhood poverty are lower than traditional operationalizations.

Although the tipping points for dichotomous, three- and four- category operationalizations of neighborhood poverty are comparable, I next examine which of these models explain the most variation in educational attainment. Using Wooldridge's (2010) multilevel application of Vuong's (1989) likelihood ratio test for non-nested models, I compared the variation explained by each of these models. The model fit of these three models are statistically indistinguishable. Hence, the most parsimonious model—the dichotomous tipping point—is preferable.

Despite the consistency of these findings, they contradict commonly held conceptions of neighborhood poverty. Thus, to ensure their validity, I ran additional supplemental tests. First, I ran models including both a continuous measure of neighborhood poverty and a dichotomous tipping point. These models capture both a general linear effect of neighborhood poverty and a categorical distinction between impoverished and non-poor communities. Like my previous results, the model that captures the most variation in neighborhood poverty uses the 5 percent poverty threshold. Additionally, the dichotomous model without the continuous measure is preferable to the models with the continuous measure of neighborhood poverty.

Second, I consider whether the distinction between neighborhoods is less about the presence of poverty and more about the abundance of wealth. To investigate this proposition, I defined wealth as four times the federal poverty rate—a common definition in the literature (Massey 1996; Dwyer 2010). Using the same methodology as above, I find the neighborhood tipping point that explains the most variation in educational

attainment is 27 percent. That is, children who grew up in neighborhoods where less than 27 percent of the population was affluent completed less schooling than their counterparts who grew up in neighborhoods where at least 27 percent of the population was affluent.

Considerable overlap exists between residents who grew up in affluent neighborhoods and residents who grew up in neighborhoods with a poverty rate lower than 5 percent. However, they are not synonymous. Of the 512 respondents who grew up in affluent neighborhoods (communities where at least 27 percent of the population had an income 4 times that of the federal poverty rate), 315 (62 percent) were also neighborhoods with less than 5-percent poverty (see Table 3.5). Nevertheless, 197 respondents grew up in communities that both had high affluence and a poverty rate that was 5 percent or greater. Conversely, 40 respondents (2 percent of the total sample) grew up in communities with poverty rates lower than 5 percent but affluence rates lower than 27 percent. Given that these communities are not identical, I can test to see which of these classifications better captures variation in educational attainment. Using the Wald test, I conclude that using poverty and in particular the 5 percent poverty tipping point explains the most variation in educational attainment. This pattern suggests that it is the absence of poverty and not the abundance of affluence that is the main driver of the relationship between childhood neighborhoods and educational attainment.

Neighborhood Affluence	Neighborhood Poverty	
	<i>At Least 5% Poor</i>	<i>Less than 5% Poor</i>
<i>Less than 27% Affluent</i>	Total N = 1686 Total % = 75% Row % = 98%	Total N = 40 Total % = 2% Row % = 2%
<i>At Least 27% Affluent</i>	Total N = 197 Total % = 9% Row % = 38%	Total N = 315 Total % = 14% Row % = 62%

Table 3.5 – Childhood Neighborhood Poverty and Affluence.

In short, childhood neighborhood poverty affects educational attainment when the neighborhood poverty rate reaches 5 percent. This is radically different than the traditional tipping point of 40 percent. This finding indicates it is not the extremely poor neighborhoods that have quantifiably and categorically different atmospheres than everywhere else but those neighborhoods with hardly any poverty at all. These statistical findings have important implications on how scholars theorize and understand the neighborhood mechanisms. Yet, before I consider these implications, I turn to examining whether this alternative neighborhood tipping point influences the conclusions of demographic analyses on metropolitan poverty concentration.

3.5.2. Analysis Two: Metropolitan Trends in Poverty Concentration and Exposure

By themselves, the above results refine statistical measurements of neighborhood effects. Nevertheless, as previously discussed, this refinement might have little bearing on demographic studies of metropolitan level poverty concentration. That is, if the trends in metropolitan poverty concentration are comparable no matter which neighborhood

tipping point is utilized, then this alternative tipping point will not influence the conclusions of metropolitan level demographic studies. To examine whether the conclusions of metropolitan level analysis would be influenced by using this alternative threshold, I begin by examining descriptive statistics across time, geographic region and racial groups. The proportion of the U.S. metropolitan population living in traditionally defined impoverished neighborhoods (census tracts where at least 40 percent of the residents live at or below the federal poverty line) has always been a small proportion of the overall population. In fact, in 2010 this proportion was 4 percent of the U.S. metropolitan population. Conversely, the proportion of the U.S. metropolitan population living in neighborhoods with moderate to high levels of poverty (census tracts with a poverty rate of at least 5 percent) includes the majority of the U.S. population—76 percent in 2010.

	Poverty Concentration				Poverty Exposure			
	Traditional Measure (40%)				Alternative Measure (5%)			
	1980	1990	2000	2010	1980	1990	2000	2010
Total Population	0.03	0.04	0.03	0.04	0.70	0.70	0.71	0.76
<i>Northeast</i>	0.04	0.03	0.04	0.04	0.65	0.57	0.64	0.67
<i>Midwest</i>	0.02	0.04	0.02	0.04	0.59	0.62	0.61	0.72
<i>South</i>	0.03	0.05	0.03	0.04	0.78	0.77	0.78	0.80
<i>West</i>	0.01	0.02	0.03	0.03	0.74	0.72	0.74	0.78
White Population	0.00	0.01	0.01	0.02	0.64	0.63	0.63	0.70
Black Population	0.13	0.15	0.09	0.11	0.94	0.90	0.90	0.89
Latino Population	0.07	0.09	0.07	0.07	0.89	0.89	0.90	0.88
Poor Population	0.11	0.15	0.12	0.15	0.91	0.92	0.93	0.95

Table 3.6 – Proportions of the Metropolitan Population that Live in Poor Neighborhoods.

Additionally, over the last three decades (1980 to 2010) the proportion of the U.S. metropolitan population living in traditionally defined “poor” census tracts has remained consistent, while the proportion living in tracts with at least 5-percent poverty has risen 6 percentage points (see Table 3.6). Likewise, when examining trends across regions, these two measures are dissimilar. Generally speaking, the proportion of metropolitan residents who live in poor tracts, as defined by the traditional measure, is consistent across all four U.S. regions. Conversely, in 2010 the proportion of residents in tracts with at least 5 percent poverty is 13 percentage points higher in the South than the Northeast (see Table 3.6). The difference between these two measures highlights their conceptual and empirical distinctions.

Nevertheless, these two measures of poor neighborhoods do not result in dissimilar metropolitan trends for all demographic trends. Specifically, temporal trends by racial groups and poverty status are comparable across both tipping points. Using both definitions of poor neighborhoods, the proportion of White metropolitan dwellers living in impoverished neighborhoods increased from 1980 to 2010. Conversely, for both definitions, the proportion of Black metropolitan residents living in poor tracts decreased over time although Blacks remain more likely than other racial groups to live in impoverished neighborhoods. Finally, across the time period, the proportion of Latinos in poor tracts remained consistent. Using either measure, Latinos are slightly less likely than Blacks to live in poor neighborhoods, but more likely than Whites. Similarly, for both measures, the proportion of the poor residents living in poor neighborhoods increased over time—denoting increasing geographic segregation of the poor.

These descriptive statistics provide a helpful glimpse at the similarities and differences between these two operationalizations of neighborhood poverty. Yet, to further explore their distinctions, I mirror previous demographic studies (e.g. Lichter, Parisi and Taquino 2012; Iceland and Hernandez 2016) and examine changes in the proportion of the metropolitan population living in poor tracts while holding other metropolitan characteristics constant. I begin with a cross-sectional model using the 2010 data. Using the traditional tipping point (40 percent poor), metropolitan poverty concentration positively correlates with metropolitan poverty and owner occupancy rate (see Table 3.7). However, when utilizing the alternative tipping point (5 percent poverty), these relationships do not exist.

Additionally, using the traditional definition of impoverished neighborhoods, metropolitan poverty concentration negatively correlates with median income and the South and West regions. Yet, when using the alternative definition, these correlations are positive. Moreover, unlike the traditional tipping point, when the alternative threshold is used results indicate a statistically significant relationship between metropolitan poverty exposure and the following: metropolitan nativity rate, the proportion with bachelor's degrees and the total population. In short, findings indicate stark variation in the direction and magnitude of respective coefficients depending on which operationalization of neighborhood poverty was used.

	Poverty Concentration		Poverty Exposure	
	<i>Traditional Measure (40%)</i>		<i>Alternative Measure (5%)</i>	
	2010	1980-2010	2010	1980-2010
Poverty Rate	1.13 (0.09)*	0.78 (0.06)*	0.13 (0.07)	0.25 (0.04)*
Racial Groups				
<i>White</i>	-0.07 (0.13)	0.21 (0.29)	0.17 (0.11)	-0.07 (0.22)
<i>Black</i>	-0.08 (0.09)	0.15 (0.23)	0.04 (0.07)	0.35 (0.17)*
<i>Latino</i>	0.01 (0.12)	-0.11 (0.25)	-0.02 (0.10)	0.02 (0.19)
Proportion in Central City	-0.00 (0.05)	-0.15 (0.08)	-0.08 (0.04)	-0.05 (0.04)
Owner Occupancy Rate	0.20 (0.05)*	0.01 (0.05)	-0.08 (0.05)	-0.06 (0.04)
Nativity Proportion	0.03 (0.09)	-0.06 (0.10)	-0.21 (0.08)*	-0.07 (0.08)
Proportion with Bachelor's Degree	0.10 (0.06)	-0.06 (0.08)	-0.11 (0.05)*	0.16 (0.06)*
Median Income, transformed	-0.40 (0.11)*	-0.32 (0.06)*	0.58 (0.09)*	0.52 (0.05)*
Population Size, transformed	0.03 (0.05)	0.02 (0.02)	0.15 (0.04)*	0.07 (0.01)*
Region (<i>Ref. Northeast</i>)				
<i>Midwest</i>	-0.16 (0.13)		0.18 (0.11)	
<i>South</i>	-0.29 (0.13)*		0.62 (0.11)*	
<i>West</i>	-0.45 (0.13)*		0.56 (0.11)*	
Year (<i>Ref. 1980</i>)				
1990		-0.14 (0.07)*		-0.06 (0.05)
2000		-0.27 (0.15)		0.00 (0.12)
2010		-0.38 (0.15)*		-0.06 (0.11)
Constant	0.26 (0.11)	0.20 (0.09)	-0.43 (0.09)	0.03 (0.07)
Number of Metros	339	339	339	339
Adjusted/Between R ²	0.5930	0.2647	0.7130	0.4235

Table 3.7 – Regressions Predicting Proportion of Metropolitan Population Living in Impoverished Neighborhoods: Ordinary Least Squares and Fixed Effects Models.

The differences between the two neighborhood thresholds persist in the longitudinal fixed effects model. These models predict change in the proportion of the metropolitan population living in poor neighborhoods from 1980 to 2010. Using both the traditional and alternative tipping points, these models indicate increases in metropolitan poverty rates over time positively correlate with increases in the proportion of the metropolitan population living in poor neighborhoods. Yet, the similarities between the

models end there. Using the alternative threshold for neighborhood poverty, increases in the metropolitan Black proportion, college educated population and total population positively correlates with increases in poverty concentration. Yet, none of these relationships exists in the model employing the traditional tipping point. Furthermore, using the traditional measure of poor neighborhoods, metropolitan median household income has a *negative* relationship with poverty concentration. However, when the alternative measure is used this relationship is *positive*. Taken together, these fixed effects models corroborate the previous evidence that these two operationalizations of neighborhood poverty produce distinct trends in metropolitan proportions living in poor neighborhoods.

3.6. Discussion

The present study's thorough examination of possible neighborhood tipping points provides a refined set of statistical tools for scholars to utilize when measuring neighborhood effects and metropolitan level exposures to neighborhood poverty. However, it also illuminates additional understudied neighborhood mechanisms and metropolitan dynamics that contribute to neighborhood effects and inequality. First, this research finding that neighborhoods with less than 5 percent poverty have categorically distinct impacts on their residents' educational attainment draws attention to the role privileged communities have in perpetuating socioeconomic inequality. Yet, little is known about the neighborhood level mechanisms in non-poor communities that facilitate this relationship. Without additional research on these non-poor communities, the present

study is unable to uncover specific mechanisms contributing to the observed neighborhood effects. Nevertheless, this work highlights the need for future research to expand the neighborhood level dynamics examined in neighborhood effects studies.

Second, this study illuminates additional metropolitan level dynamics that scholars should consider when researching metropolitan poverty concentration. For example, when using the 5 percent poverty threshold to define poor neighborhoods, higher levels of metropolitan median income correlate with more residents living in poor neighborhoods. At first, this seems counterintuitive. Why would richer metropolitan areas have more people living in poor neighborhoods? However, when the definition of poor neighborhoods under the alternative tipping point is considered, these findings begin to make more sense. As discussed above, using a 5 percent tipping point to distinguish poor and non-poor neighborhoods suggests that the categorical distinction in neighborhoods is between a select few neighborhoods that have virtually no poverty and the vast majority of neighborhoods that have moderate to high levels of poverty. Thus, higher proportions of metropolitan residents living in “poor” neighborhoods as defined by the alternative definition, means a lower proportion of residents are able to live in the elite neighborhoods that have extremely low levels of poverty. In other words, in wealthier cities and in the Northeast, fewer residents—particularly middle class residents—live in the neighborhoods with extremely low levels of poverty. This means fewer residents are living in the neighborhoods that have positive influences on educational attainment. In other words, the spatial concentration of the non-poor in wealthier cities perpetuates educational inequality. This finding, as well as other results, become evident when the alternative tipping point for neighborhood poverty is utilized. In short, beyond

refinements to the measure of neighborhood poverty, this research illuminates additional neighborhood and metropolitan level dynamics that are perpetuating socioeconomic inequality.

3.7. Conclusion

Just like the majority of neighborhood effects studies over the last few decades (Turley 2003; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Massey et al 2013; Chetty, Hendren and Katz 2016), the findings of the present study support the notion that neighborhood context shapes residents outcomes above and beyond familial or individual characteristics. Additionally, like previous demographic scholarship (Kasarda 1993; Massey and Denton 1993; Holloway et al. 1998; Quillian 1999; Jargowsky 2003; Reardon and Bischoff 2011; Dwyer 2012; Lichter, Parisi and Taquino 2012; Firebaugh and Farrell 2016; Iceland and Hernandez 2016), this research illuminates which metropolitan characteristics drive the metropolitan proportions living in poor neighborhoods. Yet, unlike the current literature, this research does not begin with the assertion that poor neighborhoods are those with at least a 40 percent poverty rate. Instead, I empirically compare over 57,000 different operationalizations of neighborhood poverty. I use these various impoverished neighborhood operationalizations to conduct two analyses.

First, utilizing the restricted geocoded PSID data, I predict adult educational attainment using each operationalization of childhood neighborhood poverty. Findings indicate that neighborhoods with a poverty rate under 5 percent are quantifiably different

than those with a poverty rate of at least 5 percent when it comes to adult educational attainment. Second, I use this alternative definition of impoverished neighborhoods to explore metropolitan trends in poverty concentration and poverty exposure. Results demonstrate that using the alternative compared to the traditional measure of poor neighborhoods produces radically different conclusions regarding the temporal trends in poverty concentration and exposure as well as which metropolitan characteristics perpetuate their expansion.

Taken together, these results both refine how scholars measure neighborhood poverty and suggest additional neighborhood and metropolitan mechanisms should be considered when studying neighborhood inequality. When interested in how neighborhood poverty shapes residents' socioeconomic outcomes, scholars should utilize the 5 percent tipping point. Future studies should consider whether similar tipping points are applicable with other dependent variables such as health outcomes. Yet, even without these future studies, this work highlights how examining the lack of poverty instead of its excess opens unexplored conceptual and theoretical avenues for understanding how neighborhoods and metropolitan areas contribute to generational inequality. Future qualitative research should investigate the mechanisms in non-poor neighborhoods that contribute to socioeconomic inequalities.

As is true with most research, this study introduces more questions than it answers. Yet, it stands as an important first step in expanding how scholars and policymakers conceptualize neighborhood inequality. Scholars and the general public tend to presume high concentrations of poverty are the “problem.” No one can deny neighborhoods with extremely high levels of poverty are often associated with a

multitude of negative attributes like violent crime, underfunded schools and limited economic development. Yet, what this research illuminates is addressing these detrimental consequences of concentrated poverty will require also considering how neighborhoods with low poverty levels contribute to the “problem.” Inequality, by its very nature, is never one sided. Thus, understanding and addressing neighborhood inequality requires examining both poor and non-poor communities alike.

Conclusion

When it was clear Civil Rights legislation had not rectified the persistent intergenerational hardships in Black inner-city communities, scholars revitalized DuBois' (1899) argument that the structural conditions of marginalized neighborhoods compounded family and individual disadvantages, in turn perpetuating poverty across generations (Danziger and Gottschalk 1987; Massey, Condran and Denton 1987; Wilson 1987). These claims inspired a new subset of the urban sociological literature—neighborhood effects—that empirically differentiated family and individual effects to demonstrate the unique role neighborhood conditions play in residents' socioeconomic outcomes (Brooks-Gunn et al. 1993; Turley 2003; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011; Kimbro, Denney and Panchang 2012; Sampson 2012; Massey et al 2013; Sharkey 2013; Chetty, Hendren and Katz 2016). Like these previous studies,

this dissertation provides evidence that, net of personal and familial characteristics childhood neighborhoods influence the intergenerational transmission of socioeconomic status. Nevertheless, unlike previous scholarship, the present work empirically demonstrates that the relationship between neighborhoods and socioeconomic outcomes is multifaceted and nonlinear.

Conducting an in-depth cross-national investigation into the measurement of neighborhood effects, this dissertation provides two primary contributions to this subfield. First, neighborhood characteristics in and of themselves do not determine particular outcomes. Utilizing longitudinal neighborhood data linked to geocoded surveys from the United States and Germany—the U.S. Panel Study of Income Dynamics and the German Socio-Economic Panel, my work illustrates how the magnitude of neighborhood effects depends on the national context. Since the 1920s, the United States has intentionally cultivated neighborhoods as a way to foster connection and community within the city. Correspondingly, city plans often design neighborhoods to have their own libraries, schools, post offices and commercial commerce. Given the centrality of neighborhoods to many essential services like education and access to fresh food, inequality in neighborhood institutional resources has strong influences on residents’ wellbeing. Nevertheless, in Germany, where redistributed policies are largely centralized and residents are less likely to depend solely on their local neighborhood resources, inequality in neighborhood institutional resources does not influence residents’ wellbeing. In other words, neighborhood effects are the product of both neighborhood inequality *and* the social construction of neighborhoods themselves. Thus, addressing the inequality perpetuated by neighborhood effects will likely require addressing inequality

and the role neighborhoods play in the distribution of governmental and commercial services.

The second main contribution of this dissertation is the revelation that neighborhood effects on intergenerational socioeconomic status are strongest in the most privileged neighborhoods. Previous quantitative research has compared all neighborhoods and noted the general correlation between residents' outcomes and neighborhood disadvantage (LeClere, Rogers and Peters 1997; Reijneveld 2002; Stewart, Stewart and Simons 2007; Ross and Mirowsky 2008; Bolt, Phillips and Van Kempen 2010; Nkansah-Amankra 2010; Martens et al. 2014). To explain this correlation, scholars pull from ethnographic research and interviews focused on the most disadvantaged neighborhoods (e.g. Gans 1965; Wilson 1987; Bourgois 2002; Small 2004; Wacquant 2004; Venkatesh 2008; Goffman 2014). These scholars implicitly and explicitly argue that the most disadvantaged neighborhoods negatively influence residents while their counterparts in less disadvantaged communities are not affected by their neighborhood context. Nevertheless, they do not decompose the relationship between neighborhood disadvantage and intergenerational transmission of socioeconomic status to establish whether neighborhood disadvantage is driving the observed correlations. By conducting various decompositions, I demonstrate that neighborhoods with the least disadvantage and lowest poverty rates are the most influential. Thus, addressing the issues of the effects of neighborhood inequality on residents' socioeconomic status will require understanding and addressing neighborhood mechanisms within the most privileged communities.

Like the vast majority of original research, these two primary findings evoke more questions than answers. First, what specific policies and sociohistorical conditions foster the creation of neighborhoods such that their resources shape residents' outcomes? How can redistributive policies be created so that all residents, no matter their residential location, are able to take full advantage of the opportunities they provide? Second, what mechanisms explain the relationship between the most privileged neighborhoods and residents' socioeconomic status? Scholars have conjectured that disadvantaged neighborhoods influence residents because of their social norms, networks, expectations and institutional resources. Are these same mechanisms driving the relationship between advantaged communities and residents' outcomes? How can scholars use this refined understanding of the mechanisms advancing the transmission of socioeconomic status across generations to create policy interventions that facilitate equitable opportunities across all urban residents?

Since my work is the first cross-national longitudinal analyses of neighborhood effects, existing studies do not explore these more specific questions about national policies that foster neighborhoods effects. Thus, these questions require future research. Similarly, I am unaware of research examining neighborhood effects mechanisms within “average” and “upper class” communities. Nevertheless, since my initial research questions were not specifically geared towards privileged neighborhoods, I wanted to return to the literature and confirm no such research existed—particularly in non-sociological disciplines where I am less familiar with the literature. To that end, I conducted a systematic content analysis of journal articles published between 1990 and 2015.

In my analysis, I include all articles that used the word “neighborhood” in their abstract (n=1,158). I then categorize the studies by theme and the type of neighborhoods examined in the research (see Appendix A for full discussion of methodology and findings). As expected, the vast majority of articles focused on marginalized communities—especially within the neighborhood effects subfield. Furthermore, no articles provided a systematic examination of social norms, networks, expectations or institutional resources in privileged communities. Instead, both qualitative and quantitative work pulled from qualitative studies on disadvantaged neighborhoods to substantiate their claims about the role neighborhoods have on residents’ wellbeing. However, these qualitative studies implicitly and explicitly compare marginalized communities to “average” or privileged neighborhoods without empirical data. For example, Wilson’s (1987) classic work *The Truly Disadvantaged* argues Black inner-city neighborhoods have less collective efficacy and less support for mainstream social norms than communities with more institutions and employed residents. Specifically, in his conclusion he states:

[Concentration effects] refers to the constraints and opportunities associated with living in a neighborhood in which the population is overwhelmingly socially disadvantaged—constraints and opportunities that include the kinds of ecological niches that the residents of these communities occupy in terms of access to jobs, availability of marriageable partners, and exposure to conventional role models. [...] The basic thesis is not that ghetto culture went unchecked following the removal of higher-income families in the inner city, but that the removal of these families made it more difficult to sustain the basic institutions in the inner city (including churches, stores, schools, recreational facilities, etc.) in the face of prolonged joblessness. And as the basic institutions declined, the social organization of inner-city neighborhoods (defined here to include a sense of community, positive neighborhood identification, and explicit norms and sanctions against aberrant behavior) likewise declined. (p. 144)

Wilson, however, never provides data on communities with more institutions or employed residents to establish whether his comparisons are accurate. The vast majority of urban ethnography over the last three decades have followed Wilson's lead and used unspecified reference groups as their implicit comparison category. Even Small (2004) whose ethnography of a Boston Barrio explicitly challenges Wilson's claims about the correlation between low socioeconomic status neighborhoods and residents community attachment and engagement, still indirectly compares a single marginalized community to an unspecified "normal" neighborhood. Likewise, when explaining neighborhood effects, quantitative scholars cite ethnographies of marginalized communities and compare them to an unstudied but assumed "normal" neighborhood. This gap in the literature is an avenue for future research. Yet, it is more than that. It speaks to a theoretical and methodological oversight that has biased much of urban sociological conclusions. I propose many of these oversights can be addressed by applying critical theory to the urban sociological literature on neighborhoods.

Towards A Critical Urban Theory of Neighborhoods and Their Effects

To explore what critical theory can bring to the study of neighborhoods, I start with an overview of critical theory. I then discuss what urban sociologists can learn from how critical race theory has employed critical theory in the study of structural inequality. Finally, I discuss the theoretical and methodological specifics of applying critical theory to the study of neighborhoods.

An Overview of Critical Theory

Critical theory traces its roots to the 1920s Frankfurt School, a loose alliance of scholars such as Theodor W. Adorno, Max Horkheimer and Herbert Marcuse who aimed to provide theoretical rationales for why Marx's forecasted downfall of capitalism never occurred (Agger 1991; Brenner 2009). They agreed with Marx that capitalism centralized the production of wealth at the expense of the proletariat. However, critical theorists argued that the advancement of capitalism increased workers' false consciousness, thereby preventing a unified revolt (Agger 1991). They saw Marx's underestimation of workers' false consciousness as an example of the shortcomings of positivism. In fact, they argued that social science's positivism was increasing false consciousness and the capitalist agenda. Thus, they proposed an alternative approach based on four key propositions.

First, critical theory is *theory*. Unlike many of Marx's writings which became guidebooks for progressive social movements, the critical theorists wanted their work to be unapologetically abstract. Second, critical theory is *reflexive*; continually analyzing the context and propositions of the theory itself. Third, critical theory is *critical*; illuminating how the interests of the capitalists are diametrically opposed to the proletariat. Finally, critical theory is *liberating*; believing in and pointing to the possibility of liberation and change (Brenner 2009).

Although theoretically distinctive, these four propositions are also broad enough to be applied to multiple disciplines and various social structures. Thus, in the middle of the 20th century, critical theory spread out from the Frankfurt School and was adopted in

multiple disciplines across the world. However, critical theory's skepticism of positivism slowed its incorporation into the social sciences (Brenner 2009; Carbado and Roithmayr 2014). For example, scholars have adopted critical theory to urban studies (Brenner 2009).³⁰ This scholarship wrestles with how capitalism shapes urban space. Yet, these theorists are almost entirely urban planners and designers and their work does not include empirical studies of urban phenomena or inequality. In fact, of the aforementioned 1,158 empirical articles I analyzed, none mentioned critical theory. However, this does not mean critical theory cannot assist empirical investigations of urban sociology.

Despite initial resistance, other subfields of sociology, such as race and gender, have successfully applied the basic propositions of critical theory to empirical examinations of inequality. I now turn to an examination of how these other subfields, specifically race scholarship, have incorporated critical theory into their empirical studies. By outlining the lesson learned in critical race theory, I highlight how critical urban theory can be applied to urban sociological studies of inequality.

³⁰ Critical urban theory conceptualizes capitalism and the city as inseparable. Moreover, they argue that the spread of capitalism is urbanizing the entire world such that the world is not urban and urban is now the world (Brenner 2009). Thus, critics of critical urban theory argue that critical urban theory is simply critical theory and not urban (Roy 2016). I agree with the notion that the lack of urban specificity is problematic. Nevertheless, the critique that urban theory is not specifically "urban" is not unique to critical urban theory. In fact, all urban theories have faced this same critique at one point or another (see Castells 1977[1972]; Zukin 1980; Walton 1993; Smith 1995; Gans 2009). However, the current dissertation cannot also tackle these larger arguments regarding the nature of urban theory. Hence for my purposes, I conceptualize this use of critical theory as an "urban" application of critical theory.

What Critical Urban Theory Can Learn from Critical Race Theory

Critical Race Theorists took Critical Theory's propositions about capitalism and applied them to the racial hierarchy. Just like Critical Theory's focus on the systemic injustices of capitalism, critical race theorists illuminated the structural conditions that perpetuated racism (Carbado and Roithmayr 2014; García, Gee and Jones 2016). They argued that racism is much larger than individual prejudices. Instead, racism is a social structure that reinforces White superiority and Nonwhite subordination. Shifting the focus away from the individual, critical race theory was able to draw attention to the inseparable connection between White privilege and the oppression of people of color (García, Gee and Jones 2016). Moreover, they argued for most of the 20th century that sociology's and anthropology's incorporation of the socially constructed racial categories and focus on marginalized individuals reified the racial hierarchy (Carbado and Roithmayr 2014). Thus, incorporating critical race theory into social science was not straightforward because the premises of social science inquiry were seen as antithetical to critical race theory.

Nevertheless, social scientists found ways to incorporate the propositions of critical race theory and refine their methodologies. Specifically, they used critical race theory to reflect on the historical roots of established methodologies and highlight the structural conditions of racial inequality. Sociologists began to examine how both historical and contemporary sociological theories and measures of race were shaped by colloquial beliefs about the racial hierarchy (Turner 1978; McKee 1993; Winant 2007). In particular, scholars noted that Whiteness was presumed to be "normal" and thus did not need studying. Yet, this assumption reinforced the notion that White culture was the

“standard” and communities of color were exceptions. Recognizing that White culture and norms needed to be studied challenged the privileged position of Whiteness.

Additionally, scholars applying critical race theory to the sociology of race began to focus less on individuals and more on the structural conditions shaping racism and racial inequality. For example, scholars shifted away from studying respondents’ beliefs about other racial groups to examining how implicit racial biases reinforce inequality (e.g. Emerson, Yancey and Chai 2001; Pager 2003; Royster 2003; Hochschild and Weaver 2007; Krysan Farley and Couper 2008; Krysan et al. 2009; Robnett and Feliciano 2011; Yves and Hooghe 2012). These studies highlight the pervasive and structural nature of racial inequality. Furthermore, they illuminate that “fixing” the predicament of people of color requires disturbing the implicit and explicit privileges of Whiteness.

Fully incorporating critical race theory into sociological studies of race and racial inequality is still a work in progress. Yet, lessons can be learned from the progress sociologists have made in their attempts to incorporate critical race theory into their research. First, scholars must recognize the social construction of categories and research methodologies. In fact, methodological reflexivity is arguably the longest lasting and most influential contribution critical theory has made in the social sciences (Agger 1991). Specifically, critical race theory has demonstrated that scholars need to acknowledge which groups are the implicit reference groups. Then scholars must explicitly study these groups. In doing so, the presumed normality of these groups is questioned and their privilege problematized. Second, scholars must creatively reimagine how studies can highlight the structural conditions of inequality and the interconnection of advantage and

disadvantage. Building off these lessons, I propose steps urban sociology can take to incorporate the premises of critical theory into the study of neighborhoods.

Cultivating Critical Neighborhood Theory and Scholarship

I propose that applying critical theory to the study of neighborhoods should begin by a reflexive approach to method and a deliberate attention to context. First, cultivating a reflexive methodological approach will require continually asking the question: what is the reference group and has it been studied? Whether studying impoverished Black communities in Chicago, “gayborhoods” in Berlin or gated communities in Johannesburg, scholars should consider what kind of neighborhoods are their assumed comparisons. Reference neighborhoods will shift from study to study depending on the questions being asked. Nevertheless, scholars need to be transparent about these comparisons and ensure they are making comparisons between empirical studies and not colloquial assumptions. Additionally, reflexive methodology requires never taking categories for granted but continually questioning their validity for present research questions. This is particularly true for categories that evoke normative evaluations.

Thanks to the influence of critical theory, social scientists now recognize racial and gender norms are not in of themselves “bad” or “good.” For example, conversational styles are not inherently hierarchical. Instead, they represent the vast diversity within human society. Nevertheless, in certain situations, particular conversational styles are given more power or privilege. A similar shift in discourse and theory is needed in the study of neighborhoods. Scholars frequently refer to certain neighborhoods as “bad” and

others as “good.” Instead of applying these broad labels, research needs to discuss how specific neighborhood conditions shape both positive and negative outcomes. This shift is more than just discourse. Shifting away from a one-dimensional conception of neighborhoods requires methodological and theoretical adjustments.

Second, incorporating critical theory into the study of neighborhoods will require recognizing the inseparable connection between privileged and marginalized areas. Scholarship repeatedly illuminates how historical and contemporary policies and preferences create neighborhood inequalities (e.g. Jackson 1985; Massey and Denton 1993; Gotham 2002; Pattillo 2007). Yet, when studying neighborhood effects, scholars focus on what is lacking in marginalized communities and ignore the connection between these deficits and the larger context that has created both the concentration of privilege and the concentration of disadvantage. Solely focusing on the problems of impoverished communities inadvertently gives the impression these difficulties can be addressed without changing advantaged communities or the social structures that deem some neighborhoods advantaged and others disadvantaged. Cultivating a critical urban theory of neighborhoods will entail being critical of the social structures and systems that perpetuate neighborhood inequality and illuminating the inextricable fate of affluent and poor communities.

Moving Out of the Ivory Tower and Into the Neighborhood

Since the 1920s, a fundamental premise of critical theory is to illuminate the possibilities for change in the world. This dissertation and its call to cultivate a critical urban theory of neighborhoods will be meaningless if it does not also inspire shifts in urban policy.

However, I perceive many of the practical implications of this work as yet to come. The findings of this dissertation, which are primarily methodological in nature, build a foundation for new ways of studying urban neighborhoods. Thus, the research that builds upon this methodology will illuminate the structural conditions of inequality and, in doing so, reveal practical implications for urban policy. Yet, even without this future research, the present research encourages urban policy interventions to shift their perspective.

Transforming the Urban Policy Agenda

Like urban research more broadly, urban policy needs to consider how privileged residents and neighborhoods are contributing to urban inequality. To illuminate how this shift in perspective would entail new policy interventions, I consider three types of urban policies that seek to address neighborhood inequality: neighborhood institutional development, racial and socioeconomic desegregation and housing accessibility.

As noted in this dissertation, neighborhoods vary in their level of institutional development—especially in the United States. Moreover, as demonstrated in the first chapter of this dissertation, neighborhood inequality perpetuates the transmission of

socioeconomic status across generations. Thus, urban policy agendas often reflect a desire to improve the resources of neighborhoods with limited commerce, deteriorating infrastructure and failing schools. The most common approach for enhancing neighborhood resources is encouraging local communities to apply for grants through the federal Housing and Urban Development Department or non-profit development corporations such as the Local Initiatives Support Corporation. For example, like the majority of Houston's Black and Brown neighborhoods, my neighborhood has no enclosed gutters. Instead, open ditches serve as the only drainage system. Thus, flooding is a constant issue quickening the erosion of streets and homes. To address these infrastructural concerns, my community has applied for several small grants (usually between \$6,000 and \$10,000). These small community initiative grants have made some improvements in the quality of our streets. However, the larger question is why majority White and upper-class communities have city funded infrastructure and marginalized communities must apply for grants to receive these services. Instead of waiting on disadvantaged communities to apply for grants, federal and local policy should tax cities who continue to reproduce neighborhood inequality and reward cities that seek to create equitable resources across all neighborhoods.

A second central concern of urban policy is racial and socioeconomic segregation. Policy regulating real estate steering and public housing voucher programs have been utilized to ensure marginalized populations have access to various neighborhoods. Yet, little has been done to encourage the distribution of upper-class and White residents across all neighborhoods. Unlike their poorer and Nonwhite counterparts, upper-class White residents know little about neighborhoods across their own cities (Krysan and

Bader 2009). Additionally, they are repeatedly steered by real estate agents and their social networks to consider certain White neighborhoods without ever considering housing in Nonwhite areas. For segregation to decline, all residents—including the most privileged—need equal access to all neighborhoods. Hence, incentives and regulations need to be focused on deconcentrating the most advantaged residents.

Finally, a central concern of urban policy is quality and affordable housing. Scholars and activists note that in marginalized communities land lords often allow houses to fall into disrepair, putting their tenants' health at risk. Moreover, when the land value of these communities becomes low enough, developers will buy large swaths of land to build high-end homes. This redevelopment is often too expensive for current residents who then must move. The pattern of disinvestment and redevelopment continues to repeat itself in U.S. cities. Yet, instead of addressing these larger structural issues, policies are often aimed at micro solutions like requiring developers to provide a few units at affordable rates. However, the larger structural issue traces back to the valuing of urban land based on neighborhood composition.

Unlike Germany and other Western European countries, where housing is a right, in the United States real estate is a commodity. In fact, it is most Americans primary way of accumulating wealth. Moreover, for the last century, home values have been determined in large part by their neighborhood context—specifically neighborhood racial composition (Howell and Korver-Glenn 2016). On the whole, landlords and homeowners in White communities accumulate wealth over time while their counterparts in Nonwhite communities lose wealth. Hence, landlords and homeowners in Nonwhite communities struggle to secure financing for home repairs. Thus, urban policy focusing on fair housing

should consider how land and homes are evaluated and the use of housing as a commodity instead of a right. Possible interventions could include new regulations on the appraisal industry that ensure neighborhood racial composition does not influence house values as well as adjustments to the tax code to stop privileging homeowners over renters.

As these three examples illustrate, a shift in measurement leads to shifts in theory which in turn transforms policy. Shifting the focus of urban policy such that inequality and not just poverty is of central concern opens new possibilities for how urban scholars and activists can foster equal opportunities among all urban residents. Yet, it also requires reflexivity and sacrifice. Urban sociology has been built around the idea that neighborhoods matter and structural constraints perpetuate poverty. The methodologies and measures used by urban sociologists reify these assertions. Now it is time that we problematize inequality and the political centrality of neighborhoods. In doing so, my hope is that urban sociology might contribute to the creation of more just and livable cities for all.

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Appendix A

A Systematic Review of Neighborhood Literature

To illuminate whether the existing sociological literature on neighborhoods included work on neighborhood effects mechanisms within privileged neighborhoods, I conducted a contextual analysis of articles written on neighborhoods from 1990 to 2015. Since the vast majority of neighborhood effects literature was published after Wilson's (1987) *The Truly Disadvantaged*, I chose a time frame that enabled me to capture the vast majority of the scholarship on neighborhood effects. I selected abstracts from the eleven most prominent sociology and urban studies journals. Defining "prominent" by their impact factor, I selected the four sociology journals with the highest impact factors: American Sociology Review (ASR); American Journal of Sociology (AJS); Social Forces and Social Problems. Likewise, I selected seven urban studies journals that publish urban sociological research including: International Journal of Urban and Regional Research; Urban Studies; Cities: International Journal of Urban Policy and Planning (IJURR); City: Analysis of Urban Trends, Culture Theory, Policy, Action; City and Community; Urban Affairs Review; and Journal of Urban Affairs. I selected all articles published between January 1990 and December 2015 that had the word "neighborhood" in their abstract.³¹ In total, 1,158 abstracts were selected.

³¹ For consistency across British and American publications, I used the American spellings of words like neighborhood and marginalized.

Selecting all abstracts with the word “neighborhood” enabled me to not only examine the neighborhood effects literature but the neighborhood literature more broadly. Yet, to identify the key theme of each study, I created an automated computer command to read through the abstracts for terms like “neighborhood effects,” “neighborhoods influence,” “neighborhood attainment,” “spatial/residential mobility,” “neighborhood change,” “neighborhood redevelopment,” “attachment,” “collective efficiency,” “belonging,” “segregation,” etc. I then categorized these codes into six themes: neighborhood effects, neighborhood attainment, neighborhood attachment, neighborhood change, segregation and other. Neighborhood effects studies looked at the influence neighborhoods have on residents. Neighborhood attainment research examined the factors leading to residents’ living in particular types of neighborhoods. Neighborhood attachment studies explored residents’ sense of belonging and involvement in neighborhood organizations. The neighborhood change category included studies exploring the redevelopment or demographic shifts in particular neighborhoods while the segregation classification included studies looking at overall patterns of neighborhood segmentation along race or socioeconomic lines. Finally the “other” category included a variety of studies, the majority of which were written by non-sociologists, looking at topics such as urban politics or economics. Although the vast majority of studies (93 percent) were only given one classification, some encompassed more than one. For example, studies looking at how neighborhood attachment influences neighborhood attainment or how neighborhood change effects neighborhood attachment.

Using this classification scheme, neighborhood effects studies make up 17 percent (197 articles). However, they are more common in the mainstream sociology journals

(i.e. ASR, AJS, Social Forces and Social Problems) than the urban studies journals. In fact, 43 percent of AJS's abstracts and 36 percent of ASR's abstracts are neighborhood effects studies. This is compared to the urban studies journals like *City and Cities* in which only 6 and 8 percent respectively of the studies were testing neighborhood effects. Likewise, segregation was another common theme in the sociological main stream journals (24 percent of AJS' articles and 27 percent of ASR's articles). Conversely, articles about neighborhood change are common in the urban studies journals (e.g. 31 percent of *Cities'* articles and 25 percent of *City's* articles) but uncommon in the mainstream sociology journals (0 percent of AJS' articles and 4 percent of ASR articles). This illustrates disciplinary differences across urban studies. Sociology focuses on how neighborhoods reinforce inequality amongst residents while geography, economics and political science are more interested in neighborhood change. Yet, this also illuminates, even more than other thematic categories, that neighborhood effects literature is situated within the sociological literature. Given that the neighborhood effects literature is largely written by sociologists, it is less likely that research exists on average or upper-class neighborhoods or on broader context shaping neighborhood effects that is not being cited by the other sociological work.

Journal	Total N	Theme of Neighborhood Study				
		Effects	Attainment	Attachment	Change	Segregation Other
American Journal of Sociology	42	0.43	0.07	0.02	0.00	0.24 0.33
American Sociological Review	45	0.36	0.13	0.11	0.04	0.27 0.29
Cities	120	0.08	0.01	0.03	0.31	0.09 0.53
City	36	0.06	0.00	0.00	0.25	0.17 0.64
City and Community	97	0.11	0.05	0.07	0.31	0.23 0.34
IJURR	72	0.07	0.00	0.04	0.29	0.15 0.50
Journal of Urban Affairs	166	0.09	0.01	0.04	0.22	0.14 0.52
Social Forces	79	0.24	0.09	0.09	0.03	0.23 0.43
Social Problems	58	0.26	0.05	0.03	0.10	0.22 0.43
Urban Affairs Review	137	0.15	0.01	0.05	0.25	0.09 0.50
Urban Studies	306	0.20	0.05	0.04	0.19	0.14 0.45
Total	1158	0.17	0.04	0.05	0.20	0.16 0.46

Table A.1 – Neighborhood Abstracts from 1990 to 2015 by Journal Title and Study Theme.

Nevertheless, to systematically explore what kind of neighborhoods were studied. I ran a second computer command looking for neighborhood descriptors such as “impoverished neighborhood,” “Black inner-city community,” “disadvantaged areas,” “White majority communities,” “high-income neighborhoods.” I used over 100 different phrases that identified specific neighborhood types. I then classified these neighborhoods into marginalized or dominant communities. Not all abstracts denoted a specific neighborhood type but the mainstream sociology journals were more likely to do so. In fact, over half of the pieces in ASR describe the type of neighborhoods studied in their abstracts. Of the abstracts that specified the type of neighborhood they researched, the overwhelming majority of them (96 percent) studied a marginalized neighborhood (e.g. disadvantaged, majority Black, Latino or immigrant). Fourteen percent mentioned studying both marginalized and dominant communities. The majority of these were quantitative studies comparing residents across all neighborhood types. In fact, only 4 percent of the abstracts mentioned dominant neighborhoods (e.g. predominantly affluent, White areas) without also mentioning marginalized neighborhoods. When only considering the studies on neighborhood effects, these patterns are even more dramatic. In fact only seven neighborhood effects articles specify dominant neighborhoods in their abstracts and only two articles specified dominant neighborhoods without also specifying marginalized neighborhoods.

Journal	Total N	Specified Type	Marginal	Dominant	Both
American Journal of Sociology	42	0.43	0.56	0.17	0.28
American Sociological Review	45	0.51	0.87	0.00	0.13
Cities	120	0.08	0.80	0.10	0.10
City	36	0.06	1.00	0.00	0.00
City and Community	97	0.30	0.83	0.03	0.14
IJURR	72	0.25	0.94	0.06	0.00
Journal of Urban Affairs	166	0.27	0.89	0.02	0.09
Social Forces	79	0.39	0.81	0.00	0.19
Social Problems	58	0.41	0.75	0.04	0.21
Urban Affairs Review	137	0.28	0.82	0.05	0.13
Urban Studies	306	0.14	0.86	0.02	0.12
Total	1158	0.24	0.82	0.04	0.14

Table A.2 – All Neighborhood Abstracts by Journal Title and Specified Neighborhood Type.

Using these categories, I read the front end of all the neighborhood effects articles that specified dominant neighborhoods in their abstracts. Many of these articles were discussing Move To Opportunity (MTO) or other similar programs that relocated public housing residents to high-income neighborhoods. Although each of these theoretical backgrounds compared dominant and marginalized communities, they relied solely on evidence from studies of marginalized communities when explaining the rationales for why neighborhoods influence residents. In other words, these scholars pulled from ethnographic research on marginalized communities that illustrated particular social norms and then inferred the opposite was true in dominant areas. I then examined the non-neighborhood effects literature that specified dominant neighborhoods in their abstracts. Although some of these studies were ethnographic studies on dominant communities (e.g. gated communities in South America), none looked at how these communities might be shaping residents' socioeconomic or physical wellbeing. Instead, they focused on residents motivations for living in gated communities and interactions

with residents' outside their gated neighborhoods. In short, this systematic review of the neighborhood literature confirmed my previous observation that little is known about the mechanisms operating in "average" or "upper-class" communities to shape residents' outcomes.

Journal	Total N	Specified Type	Marginal	Dominant	Both
American Journal of Sociology	18	0.28	0.60	0.20	0.20
American Sociological Review	16	0.50	0.88	0.00	0.13
Cities	10	0.00	0.00	0.00	0.00
City	2	0.00	0.00	0.00	0.00
City and Community	11	0.36	1.00	0.00	0.00
IJURR	5	0.40	1.00	0.00	0.00
Journal of Urban Affairs	15	0.33	0.80	0.00	0.20
Social Forces	19	0.32	1.00	0.00	0.00
Social Problems	15	0.47	0.86	0.00	0.14
Urban Affairs Review	20	0.15	0.67	0.33	0.00
Urban Studies	62	0.11	0.86	0.00	0.14
Total	193	0.24	0.85	0.04	0.11

Table A.3 – Neighborhood Effects Abstracts by Journal Title and Specified Neighborhood Type.